Author:
Stockton, Isabel

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The role of non-wage job characteristics in labour markets characterised by search frictions

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Isabel M. Stockton

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Abstract

In this thesis, I analyse the interactions of wages and job attributes in labour markets in the UK and Germany. In the first chapter, I estimate female workers' marginal willingness to pay to reduce commuting distance using a flexible stratified partial likelihood model on a large administrative dataset for West Germany. I find that men's marginal willingness to pay as a share of their wage is 20% lower than childless women's, which nearly doubles after the birth of a child. In sensitivity analyses I discuss the role of childcare, housing cost, regional structure and part-time work. In the second chapter, I use innovative survey data to analyse workers' willingness to pay for schedule autonomy. I find that whilst childless women's willingness to pay is lower than childless men's, motherhood (but not fatherhood) significantly increases willingness to pay. Moreover, I find that workers who enjoy schedule autonomy perceive their wages to be fairer, feel a greater sense of loyalty towards their employer and are more likely to report making an "above-and-beyond" effort at work. In the third chapter, I analyse the impact of a series of minimum wage increases in the UK on a range of schedule amenities. Using region-industry level data, I find that in regions and industries most impacted by minimum wage policies, the prevalence of zero-hours contracts increases. The effect is robust to a range of specification choices, including a dynamic Arellano-Bond model. I find less strong evidence of a decrease in schedule amenities valued by workers including flexitime. The effects are concentrated in industries with a majority of women workers.

Acknowledgements and Dedication

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To my grandmother, Christel Kräuter.

List of Abbreviations

- LFS Labour Force Survey
- ASHE Annual Survey of Hours and Earnings
- NLW National Living Wage
- NMW National Minimum Wage
- GVA Gross Value Added
- OLS Ordinary Least Squares
- IEB Integrated Employment Biographies
- GDP Gross Domestic Product
- SPL Stratified Partial Likelihood
- IAB Institute for Employment Research (*Institut für Arbeitsmarkt- und Berufsforschung*)
- AME Average Marginal Effect
- L. Lagged variable
- FE Fixed Effect
- A.-Bond Arellano-Bond (model)
- RPI Retail Price Index

Introduction

This thesis is concerned with the interplay of wages and job attributes, in particular those related to the time cost of work. This includes fixed costs in the form of commuting, as well as schedule types that make it easier to combine market and non-market work by offering additional flexibility to workers, or that make it more difficult by reducing the predictability of working hours.

These amenities reduce the opportunity costs of market work in terms of lost home production. Within the household, willingness to trade them off against wages reflects preferences and drives choices on the division of labour between different household members. In the market, it contributes to gender and motherhood wage gaps. Whereas the popular discourse on pay gaps often paints a stark dichotomy between unrestricted free choice on the one hand, and externally imposed obstacles on the other, economic analysis makes explicit the trade-offs involved in voluntary choices. This not only provides a clearer picture of the decisions that underlie observed discrepancies in labour market outcomes but can also help design and evaluate policy measures designed to redress them.

In the first chapter, joint work with Annette Bergemann and Stephan Brunow, I consider trade-offs between wages and commuting distances with a focus on gender and parenthood. We use a large administrative dataset from Germany with multiple job spells per individual, which enables us to account for unobserved heterogeneity in a flexible way. We estimate that West German, non-university educated childless women are willing to give up .5% of their wage to reduce their commuting distance by 1km at the margin. After the birth of their first child, willingness to pay doubles. We also explore heterogeneity with regard to housing costs and childcare availability, between urban and rural areas and between full-time and part-time workers.

In the second chapter, joint work with Gerard van den Berg, I study willingness to pay for flexible working hours using innovative survey data on multiple reservation wages. We find that whilst childless women's willingness to pay is lower than than childless men's, motherhood (but not fatherhood) significantly increases willingness to pay. Moreover, we discuss the potential role of reference points in our results and test a theoretical prediction of willingness to pay affecting worker decisions beyond job offer acceptance, such as effort provision. We find that workers who enjoy schedule autonomy conditionally perceive their wages to be fairer, feel a greater sense of loyalty towards their employer and are more likely to report making an “above-and-beyond” effort at work.

In the third chapter, I analyse the impact of a set of policy changes in the UK - upratings of the National Minimum Wage and the introduction of the National Living Wage - on schedule amenities. I find that in regions and industries most impacted by minimum wage policies, the prevalence of zero-hours contracts increases, an effect which is robust to a range of specification choices, including a dynamic Arellano-Bond model. I find less strong evidence of a decrease in schedule amenities valued by workers, including flexitime.

1 There and Back Again: Women's Marginal Commuting Cost

with Annette Bergemann and Stephan Brunow

Statement on co-authorship: This chapter is joint work with Annette Bergemann and Stephan Brunow. I have made very significant contributions to all aspects of the work, including data cleaning, estimation and writing up the paper. Placement in the literature, specification search as well as heterogeneity analysis were done by myself alone. Annette Bergemann, in addition to offering guidance on all aspects of the work in her role as PhD supervisor, contributed to the development of the empirical model and the interpretation of results. Stephan Brunow contributed to data cleaning and running estimation in the later stages of the project.

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Commuting as an important element for the functioning of the labour market has received renewed interest in labour economics in recent years. It is well known that workers able and willing to commute further have access to more and potentially higher-paid jobs. Some evidence has also emerged (see for example Gutierrez [2018], which was written simultaneously to our paper and Hirsch et al. [2013]) that differences in the willingness to commute between men and women might be a contributor to the gender and motherhood wage gaps. The opportunity cost of commuting is likely to be higher for women and in particular mothers with caregiving responsibilities.

This paper analysis the marginal willingness to pay to reduce commuting with a particular focus on women which can help evaluate policies to address the gender and motherhood wage gap. These include policies directly reducing the need to commute, such as telecommuting schemes and other alternative forms of workplace organisation, or policies aimed at enabling mothers to commute further, including childcare, which we discuss in this paper.

Looking beyond commuting, the issue of willingness to pay for job attributes has also been in the renewed focus of labour economics lately. New methods address some of the econometric and conceptual problems of older, hedonic estimates, which have been shown to deliver biased results in the presence of worker and firm heterogeneity as well as for markets with search frictions [Gronberg and Reed, 1994, Hwang et al., 1998]. Gender differences in willingness to pay for job attributes could contribute significantly to observed disparities in labour market behaviours and outcomes, such as participation, labour supply, job mobility and particularly wages but have not been widely studied in this respect. In this paper, we built upon the results of Gronberg and Reed [1994] and van Ommeren et al. [2000] who show that on the basis of a partial job search model the willingness to pay for a job attribute can be estimated with the aid of a hazard rate models to leave a job.

Using a large administrative dataset on job durations and commuting distance for West Germany we estimate female and male workers' marginal willingness to pay to reduce commuting distance with a flexible Cox Model where we also take account of unobserved heterogeneity by using a stratified model. We find a marginal willingness to pay for childless women of €0.31 to reduce commuting distance by one kilometre at their mean daily wage. Willingness to pay nearly doubles after the first birth from 0.52% to 1.03% of the daily wage, but since the daily wage drops at the same time, the Euro value at the mean wage for mothers of young children at €0.55 is less than twice its counterpart for childless women. Compared to other high-income countries, the German unadjusted gender and motherhood wage gap [Grimshaw and Rubery, 2015] are particularly high, making the issue particularly salient. While the differences we find, in particularly those associated with childbirth, are qualitatively striking, our evidence suggests that commuting costs overall are not large enough for these differences to explain a large fraction of gender and motherhood pay gaps. However, the mechanisms underlying differences in commuting cost, notably differences in the opportunity cost of time, are likely to play a role in a range of other labour market inequalities, not just commuting patterns.

The paper is structured as follows: The first section provides an overview of the relevant literature. Subsequently, we specify a partial-equilibrium model of job search with jobs characterised by a wage and a commuting distance. Particular reference is made to to differences by gender and the impact of the regional labour market situation, and a tractable estimator of marginal willingness to pay to reduce commuting distance is derived, following Van Ommeren et al. [2000]. We then present and discuss an estimate of marginal commuting cost using a Cox model on an administrative linked employer-employee dataset.

1.1 Literature Review

Our paper connects to a number of different strands of literature: it considers commuting decisions in the context of a search labour market with a focus on gender and motherhood.

Methodologically, early studies on compensating wage differentials for job amenities augmented Mincerian wage regressions on education, labour market experience and tenure with measures of different job attributes, in particular risk of injury or death [Brown, 1980, Viscusi, 1978]. In addition to the mixed evidence it delivered, the hedonic approach has a number of theoretical weaknesses. Search frictions and unobserved productivity differences between workers and firms are not taken into account, leading to a substantial downward bias of OLS estimation of worker's marginal willingness to pay [Hwang et al., 1998]. One way to reduce bias from unobserved worker and firm heterogeneity is by instrumenting for non-wage job characteristics [e.g. Olson, 2002, for health insurance coverage]. However, even in the rare case that a credible instrument is available, the bias deriving from search friction still remains when using Mincerian wage regression in order to estimate compensating wage differentials.

Concerns about biased hedonic estimates gave momentum to the development of alternative estimation techniques. These methods are closely related to search models, putting quit data and job spell durations in the focus. They also improve on very early models of wage premia in a search environment [Khandker, 1988] by enabling the analysis of continuous job attributes and addressing issues such as sensitivity to outliers and measurement error [Eckstein and van den Berg, 2007]. The central idea is that more desirable job attributes decrease the probability of quitting in a search model with multi-dimensional jobs [Clark, 2001].

Marginal willingness to pay for non-binary[1] job attributes cannot be directly recov-

[1] Some authors [e.g. Villanueva, 2007] discuss a *marginal* willingness to pay for binary job attributes. However, utility should change infra-marginally with the presence of a binary job attribute and would

ered from observed search processes from unemployment, which were at the centre of the first generation of search models. But on-the-job search was an early and intuitive extension [Burdett, 1978] which Gronberg and Reed [1994] used to derive a measure of willingness to pay that is robust to search frictions. As long as the problem is stationary[2], this preserves the tractability and many of the core predictions of the original model.

In the labour search literature, differences in the job search process of male and female workers have been formalised in a number of different ways: as differences in job offer arrival rates, in job destruction rates, or in parameters governing exits into non-participation. Our model allows for all these differences. In addition, potential differences in the marginal willingness to pay to reduce commuting, derives in our model from different instantaneous utility functions over wages and commuting distances between men and women.

In Bowlus [1997] and Bowlus and Grogan [2009], parameter estimates such as job finding and job destruction rates exhibit significant differences across genders. These differences are heterogeneous – in several instances, they even change sign – across education levels, highlighting the importance of interactions of gender with other determinants of labour market behaviour. In our main analysis, we exclude university graduates to create a more homogeneous group in terms of education. We also explore various other types of heterogeneity, analysing the role of housing costs and childcare availability as well as differences between urban and rural areas and between full-time and part-time workers.

A number of different economic and behavioural explanations have been proposed to explain these differences in model parameters. They include differences in the value of non-market time related to home production and especially childcare, an explanation highly relevant to our own application. Other authors have stressed differences in search methods [Kuhn and Skuterud, 2004], in the propensity to bargain over wages [Hall and Krueger, 2012], and in willingness to relocate for a job [Blackaby et al., 2005]. Still others highlight gender-segregated professional networks [Montgomery, 1991, Mencken and Winfield, 2000] and discrimination [Bowlus and Eckstein, 2002, Flabbi, 2010].

Moving back to the literature on trade-offs between wages and job attributes, studies

consequently not be continuous, much less differentiable.

[2] The search environment does not depend on the current job, nor on the elapsed duration of the spell.

such as Reed and Dahlquist [1994], DeLeire and Levy [2004] and Felfe [2012] examine gender differences in willingness to pay for attributes including workplace safety, type of tasks, promotion opportunities and different work schedule arrangements. They use duration models or conditional logit models, and studied the effect of job attributes on job-to-job mobility as well as mobility between labour market states around childbirth.

Taber and Vejlin [2016] use a structural model to determine the importance of compensating differentials as a whole in explaining workers' utility, wages and job mobility as opposed to other, competing explanations. Their model is identified without data on observed job characteristics and instead attributes voluntary transitions with wage cuts to compensating differentials. Using Danish matched employer-employee data for the period of 1985-2003, they find that job attributes, along with search frictions, explain a substantial part of the variation in utility and turnover. However, compensating differentials are less important for college-educated men than for all other gender-education groups.

Commuting distance as a job attribute is increasingly receiving attention in the context of gender differences, but few studies have been published so far. Van Ommeren and Fosgerau [2009] apply a combination of a job search model with a short-run model of commuting *time* for a fixed *distance* to the commuting decision. The authors estimate the marginal cost of an hour's additional commuting in the Netherlands at about twice the hourly wage using a discrete-time framework, imposing quite strong functional form assumptions and self-reported information on workers search as outcome variable. This is much higher than estimates from models which focus on time costs only, which leads the authors to conclude that monetary costs make up about half of the overall amount at the margin. They find economically significantly differences between gender, but not statistically significant differences.

Our approach is similar in spirit to Van Ommeren et al. [2000] and Russo et al. [2012], who use parametric or semi-parametric duration models in continuous time to estimate parameters from job spell data.[3] We complement their results by using a 10% sample of all German employees, allowing us to examine more general patterns than their analysis of a single large employer, with our particular focus on gender differences. At the same time, we retain the advantages of administrative data with a panel dimension, which allows us to account for individual heterogeneity in a more flexible way than Van Ommeren et al. [2000].

[3]We will discuss this in more detail when presenting the theory underlying our own work.

By its nature, commuting distance arises as the result of decisions in at least two markets – one in the housing market and one in the labour market. The literature in labour economics has usually treated residential location as exogenous, turning commuting distance into a job or match attribute. Conversely, in urban economics, commuting distance is studied as a result not of job choice, but of residential location decisions. In this tradition, compensation for commuting distance is available in the housing market through lower house prices, while the wage is usually considered exogenous.

Early models of commuting in urban economics considered a household with one worker, often quietly presumed to be male. In an early contribution White [1986] uses an OLS regression of commuting time on income and demographic variables, with a focus on gender differences, in particular women's shorter commutes, a highly persistent finding across time and space [for Germany, see Auspurg and Schönholzer, 2013]. However, her approach is unable to account for a number of unobserved differences between households classified as male and female-headed, and is vulnerable to simultaneity bias. Models such as Black et al. [2014] extend the traditional framework to accommodate household decision-making.

Timothy and Wheaton [2001] showed that the one-dimensional wage gradient predicted by the monocentric models[4] [White, 1986, Alonso, 1960, 1964] common in the early urban and regional literature on commuting does not fit the observed wage variation in US Census data well. To explain the observed multidimensional variation, they therefore present a multinodal model where jobs are spread out over multiple locations across a city. The spatial focus of the model can explain interactions between housing and labour markets, which most models in labour economics ignore. On the other hand, workers and firms are assumed to be matched without costly search, a substantial limitation from the labour-economic point of view.

Gutierrez [2018] find that among mixed-sex married couples in the United States, one-tenth of the gender pay gap among childless workers and more than a fifth of the motherhood pay gap are explained by commuting differences. Their modelling approach emphasises a different set of decisions from ours: Given the high mobility in the US, they explicitly model residential location decisions, relying on a monocentric model of the city with a central business district and a gradient of wages and housing costs. They also explicitly model household fertility and labour supply decisions, imposing a gendered division of labour where only wives can engage in household and child-rearing

[4] Models of a city with a single central business district where jobs are located.

tasks. On the other hand, search frictions are not modelled and since the data does not have a longitudinal dimension, individual heterogeneity cannot be modelled in as flexible a way.

Rouwendal and Rietveld [1994] and Rouwendal [1999] develop spatial models of labour market search in the same spirit as our theoretical model. However, their empirical strategy differs substantially from ours. Rouwendal and Rietveld [1994] parametrize a distribution function and estimate it on a short panel. Rouwendal [1999] estimates a full structural model on cross-sectional survey data, again relying heavily on functional form assumptions for identification and to address unobserved heterogeneity. The two papers find that the presence of children in the household decreases women's commuting distance [Rouwendal and Rietveld, 1994] and increases their marginal commuting cost [Rouwendal, 1999].[5]

In contrast, our strategy relies on a relationship derived from a partial-equilibrium search model and estimated using duration analysis on spell data. The daily precision of our dataset allows us to use the semi-parametric method of partial likelihoods, granting additional flexibility compared to strong distributional assumptions. The multiple-spell structure of the data also allows us to account more flexibly for unobserved heterogeneity.

Estimating marginal commuting cost from a partial labour market search model requires the assumption that residential location is exogenous. Exogenous does not necessarily mean fixed, but a low residential mobility is consistent with a lack of re-optimisation in response to job changes and thus, exogenous residential location. The estimated rate of household residential mobility in Germany over a period of two years is estimated to be just over ten percent, substantially lower than the UK rate and only about half of the US rate [Sánchez and Andrews, 2011]. Moreover, residential mobility has been shown to increase with educational attainment (ibid and references therein) and our sample excludes university graduates. We therefore work with a relatively immobile sample.

The studies discussed above, whether in the tradition of labour market search or urban economics, focus on longer-term job and residential location choices. There are two estimation strategies in the choice and transportation economics literatures which

[5]This evidence is consistent the *household responsibility hypothesis*, which explains differences in commuting time with the uneven division of non-market work between men and women and consequent higher productivity of women's non-market time.

estimate the value of travel time directly, without modelling job or residential location choices. On the one hand, stated preference methods study hypothetical choices from survey data. The results exhibit quite striking variations, even when similar models are applied [e.g. Calfee et al., 2001, Small et al., 2005]. It could be the case that the treatment of unobserved heterogeneity drives the stark differences in estimates, in which case the problem would be econometric in nature. Another possibility is that stated preference data could simply be an unreliable signal of underlying preferences [Hensher, 2004]. Parallel to our paper Le Barbanchon et al. [2019] are using incentivised stated preference data arising from an administrative process, whereby job seekers in France have to state the lowest wage and longest commuting distance they would be willing to accept, to the employment agency. Conditional on the characteristics of the previous job and the local labour market where the search takes place, they find a gender gap in willingness to pay to reduce commuting distance that is very close to our own results.

On the other hand, revealed preference methods analyse observed choices nested within the commuting decision, such as mode, route, or vehicle choices. For example, Brownstone et al. [2003] and Lam and Small [2001] both estimate a high marginal willingness to pay for commuting time of more than 70% of the wage rate. They circumvent the problem of biased reporting of willingness to pay, but data with sufficient variation of alternatives is not readily available, and the interpretation often extrapolates far beyond the range of commuting times actually observed. Older studies using revealed preference data also relied on strong assumptions on the shape of the utility function, as Van Ommeren et al. [2000] point out.

1.2 A Model of Job Search with Commuting

In this section, we will outline an on-the-job search model extended to two-dimensional jobs, closely following Van Ommeren et al. [2000]. Without information on rejected job offers, marginal willingness to pay for non-binary job attributes cannot be recovered from search from unemployment. Voluntary job-to-job transitions, on the other hand, do identify this parameter.

Consider an employed worker in a job with wage w and commuting distance d, who receives alternative job offers (w^*, d^*) drawn from a distribution $F(w^*, d^*)$ according to a Poisson process with arrival rate λ. Thus, an important assumption underlying the model is that residential location is exogenous to the search process. An assumption that fits to our context (see previous section).

A potential source of bias related to endogenous residential location is compensation for long commutes in the housing market as in the classical models of urban commuting to a single central business district discussed in section 1.1. Those who have long commutes live far away from the central business district and may therefore face lower housing costs. This would lead to an underestimation of marginal commuting cost in our model, since workers are compensated for part of the cost in the housing market, in addition to the labour market. We address this issue by conditioning on a measure of local housing cost.

In addition to voluntary job-to-job transitions, employment spells end for exogenous reasons at rate δ. The expected discounted stream of utility from accepting job offer (w, d) over the whole of the life course is

$$\rho R(w,d) = u(w,d) + \theta \int\int \max\{0, R(w^*, d^*) - R(w,d)\} dF(w^*, d^*) \\ + \delta(U - R(w,d))$$

where ρ is a discount parameter and U is the expected present value of unemployment. Lifetime utility is thus composed of an instantaneous component, a continuation value in case of a job switch and another continuation value in case of exogenous job loss.

The optimal strategy, as in the one-dimensional job case treated by Mortensen [1986], is myopic. The reason for this is that lifetime utility R depends on (w, d) only through instantaneous utility $u(w, d)$ and there are no transaction costs. Intuitively, whereas in a model without on-the-job search, a worker may "hold out" for a better offer, workers here have nothing to lose in accepting a job offer. They will still have an equal chance of receiving a better offer on the job.

Therefore, the worker pursues a reservation utility strategy: She accepts all job offers which offer a higher instantaneous utility than her present job, since the future stream of job offers is not affected by the job currently held. Formally, the set of job offers that are acceptable (i.e., strictly preferred to the current job) is

$$\varsigma(w,d) = \{(w^*, d^*) | u(w^*, d^*) > u(w,d)\}$$

This search and decision process leads to the following specification for the hazard rate from a job (w, d):

$$\theta(w,d) = \delta + \lambda \int_{\varsigma(w,d)} dF(w^*, d^*) = \delta + \lambda(1 - F_u(u(w,d)),$$

i.e. the rate of exit from a job is given by the rate of exogenous exits into unemployment, plus the product of the rate of arrival of alternative offers and the probability that the offer will induce the worker to switch jobs. The second expression follows by substituting the above characterisation for the set of acceptable job offers, with F_u denoting the c.d.f. of $u(w, d)$.

As stated before, lifetime utility in this model depends on the wage and the commuting distance only through instantaneous utility. Therefore, the partial derivative of the hazard rate with respect to the wage w can be expressed as

$$\frac{\partial \theta(w,d)}{\partial w} = \frac{\partial \theta(w,d)}{\partial u(w,d)} \frac{\partial u(w,d)}{\partial w}$$

Clearly, an analogous statement holds for the derivative with respect to the commuting distance d.

This, in turn, gives us the equality stated by Gronberg and Reed [1994]: the instantaneous marginal rate of substitution or marginal willingness to pay for a job attributes is equal to the ratio of the marginal derivatives of the hazard rate:

$$\frac{\frac{\partial \theta(w,d)}{\partial d}}{\frac{\partial \theta(w,d)}{\partial w}} = \frac{\frac{\partial \theta(w,d)}{\partial u(w,d)} \frac{\partial u(w,d)}{\partial d}}{\frac{\partial \theta(w,d)}{\partial u(w,d)} \frac{\partial u(w,d)}{\partial w}} = \frac{\frac{\partial u(w,d)}{\partial d}}{\frac{\partial u(w,d)}{\partial w}} \tag{1}$$

Regional Labour Market Conditions As an extension to their basic model, Van Ommeren et al. [2000] discuss the inclusion of business cycle effects in the model. They would affect the rate of arrival of job offers λ and/or the distribution $F(w, d)$ from which wage offers are drawn. Realistically, not only macroeconomic conditions at the national level should affect these two structural parameters of job search, but also regional trends, which therefore enter into the hazard rate.

In our empirical specification, we therefore include dummies for county settlement structure, as well as local unemployment and growth rates to reflect regional labour market conditions. We have also experimented with indices counting regular employment relations in the individual's county of residence and in neighbouring districts, in her occupational field, by gender. The intuition is that the individual is likely to receive offers to work in her own profession, as well as in other professions within the same occupational field, which are defined with respect to similarity of tasks performed and skills required. Results were unaffected by the inclusion of different local labour market indicators. Importantly, we assume residential location to be fixed and define the local labour market environment around it, which conserves the stationarity of the decision problem.

Gender As explained in section 1.1, a number of structural sources for different outcomes by gender in the search model have been suggested, such as different job offer arrival rates or different wage offer distributions. However, these would not affect marginal willingness to pay as a function.[6] Since the hazard rate depends on (w, d) only through the instantaneous utility $u(w, d)$, the structural source of differences in marginal willingness to pay across the (w, d)-plain has to be differences in the instantaneous utility function. The search environment is allowed to differ in many other ways for men and women, or indeed between individual workers of the same gender. These differences are captured in a very flexible way by the individual-level baseline hazard. Notable sources of such heterogeneity could be the job offer arrival rate and the distribution of wages and commuting distances offered.

As for the underlying causes of differences in instantaneous utility (which, in turn, generate differences in marginal willingness to pay), a common assumption in the literature is that women's non-market time is more productive than men's. This could be the case because they remain responsible for the bulk of household and child-rearing tasks. If social norms dictate that mothers should be nearby, for example to attend school-related meetings or events or to be available in case of emergencies, the opportunity cost of commuting time would be higher for women than for men and for mothers than for non-mothers. Other possible explanations include differences in access to a car, as suggested by Best and Lanzendorf [2005], or in the disutility from travelling due to differences in taste or perceived safety.

[6] Marginal willingness to pay *at an observed wage* may differ if, as in most estimations in the literature as well as our baseline specification, it varies in the wage. In addition to purely empirical goodness-of-fit arguments, this specification can be justified by higher opportunity costs of commuting time for high-wage workers.

1.3 Empirical Analysis

1.4 Data

The data sets used are samples of the Institute for Employment Research's *Integrated Employment Biographies*. Since the data comes from administrative social security records, they are more accurate than survey datasets commonly used in studies of commuting. For instance, they avoid problems of recall error in job spell durations and biased self-reporting of wages. The main sample consists of the inflow into employment after January 1st, 2000 until December 31st, 2013, recording employment and unemployment spells in days. Self-employment is observed only in exceptional cases, since self-employed workers do not usually contribute to national insurance. The inflow sample is constructed from a 10% sample of all individuals with a national insurance number, going back to 1975. We restrict the sample to West German workers in order to be able to use information on work experience based on the full biography and, crucially, to reliably identify the first birth for women. Since East German workers' records are only available from the early 1990s, it is difficult to distinguish first from subsequent births to East German women during our sample period. This problem is exacerbated by fertility patterns around reunification, when birth spacing in East Germany often showed a long gap, between a first child born before reunification and further children born much later.

Employment spells in the sense of the model are times in regular full- or part-time employment with full national insurance contributions, which make up just over two-thirds of the full sample. The wage information refers to daily wages. Wage income above a certain threshold, which is usually adjusted annually, is not subject to national insurance contributions and therefore censored. To alleviate bias arising from this selection, we restrict our analysis to workers without a university degree, who less frequently earn wages above this cutoff values. Some systematic underreporting of higher education is known to occur. However, education information during job spells is considered most reliable, since it is employer-reported [Fitzenberger et al., 2005]. To minimise selection bias from underreported education, we smooth education, classifying individuals as university graduates after the first reporting of a university degree, even if the recorded variable switches to vocational training or no training afterwards, based on one of Fitzenberger et al. [2005]'s correction procedures. Adding to the problem of top-coded wages, university graduates are also likely to be more geographically mobile, which makes the assumption of exogenous residential location more questionable.

Apprenticeships and marginal employment (*geringfügige Beschäftigung* with either low total earnings or on short-term contracts, which are exempt from national insurance contributions) are excluded, since job mobility behaviour for these groups is likely to follow different patterns from regular workers and data is unavailable for earlier years. We also exclude publicly sponsored employment, jobs within the context of an active labour market programme and jobs where the worker claims a mobility subsidy, since the observed wage and/or commuting distance do not adequately describe the worker's decision problem. For consistency, we also exclude jobs which switch back and forth between regular and marginal or sponsored employment. Moreover, we observe unemployment benefit claims, a number of other benefits such as early retirement programmes and participation in active labour market policies and training.

Following the majority of the literature, we measure commuting as distance. More specifically, our distance variable measures Euclidean distance between postcode area centroids. We argue that particularly for Germany distance and travel time are very closely related. Firstly, Germany's geography generates little heterogeneity in travel time for a given distance. Secondly, we control for regional structure, which captures elements of transport infrastructure. Finally, only 12% of the traffic volume associated with travel to work in Germany uses public transport [Follmer et al., 2010]. This means that public transport infrastructure is not an central determinant of travel time, again making travel time relatively more homogeneous in space. We drop observations with a distance above 100km. We conduct sensitivity analyses to make sure this does not unduly affect our results.

We measure distance between a worker's home and place of work, rather than journeys made or journeys required by the employer. Similarly to willingness to pay estimates for other job attributes, our estimates therefore take as given any adaptations that employers and/or workers use to make the attribute less onerous, such as telecommuting schemes. According to an analysis based on the *Mikrozensus* [Brenke, 2014], 8% of employees in Germany occasionally or primarily work from home, but the share was highest in high-skilled occupations which normally require a university degree and are not in our main sample. It is of course likely that many of the workers in the "occasional work from home" group complete work-related tasks at home outside of working hours, which needn't reduce the number of journeys to their place of work.

A central finding of our paper is the large effect of childbirth on women's willingness to pay to reduce commuting distance. To identify the timing of births in the data, we use a routine due to Müller and Strauch [2017], based on exits from employment into the mandatory part of maternity leave. Particularly for our sample of non-graduate women who are unlikely to have a child before entering the labour market, this is a very reliable way of identifying first births.

1.5 Specification and Model Choice

A duration model is the most direct and intuitive empirical implementation of the job search model discussed in Section 1.2. While no real-world data is ever truly in continuous time, the daily frequency of our dataset comes close. Job spells can and do start, and wages and other job characteristics can and do change, at any point during a month.

In the duration model of job mobility specified in this section, the failure event is a job ending for any reason. These include voluntary job-to-job transitions, layoffs and exits from the labour market. A job spell may be followed by a spell of missing data for several reasons: periods of full-time education, parental leave, time spent abroad and self-employment are all coded as missing. Voluntary transitions and layoffs are not unambiguously distinguishable from the data. Under the assumption that layoffs, like any exit events other than voluntary job-to-job transitions, are exogenous to the model, they will be captured by the baseline hazard. Since the estimator for marginal willingness to pay includes only partial derivatives of the hazard, the layoff risk drops out. Given that we allow the baseline hazard to vary at the individual level, this assumption is not very restrictive.

A more complete model would directly model the hazards of leaving a job for other reasons in a competing risk framework. Identification of such a model is much more difficult than in the present case [Van den Berg, 2001]. Bonhomme and Jolivet [2009] specify a model where workers are at risk for different events ending a job and let the hazards depend on individual characteristics. However, their model is quite different from ours: they study an objective, continuous latent amenity whose value is compared to a subjective, individual-specific threshold. This threshold varies by observed and unobserved worker characteristics and the comparison determines "good" and "bad" jobs. The authors then estimate this model on categorical attribute data. In contrast, we conceptualise and measure a continuous amenity, which can in principle enter the utility function in any functional form and whose offer distribution is unspecified. Also, our model can accommodate a more general form of unobserved heterogeneity.

The Stratified Cox Model Our sample contains multiple job spells per person, enabling us to account for unobserved heterogeneity using the method of Stratified Partial Likelihoods. As Ridder and Tunalı [1999] describe in their paper introducing the method, failure to account for unobserved "group" characteristics (in our case, characteristics which affect all the job durations generated by the same worker throughout her employment biography) biases coefficient estimates. It bears repeating that this is true even if these unobserved characteristics should be uncorrelated with the included regressors. It has long been known that if unobserved heterogeneity is scalar and uncorrelated with the regressors, the estimated β coefficients we use to calculate willingness to pay will be biased towards zero [Ridder, 1984, Ridder and Tunalı, 1999]. However, if unobserved heterogeneity has an arbitrary form and a fortiori in the case of a correlation between unobserved heterogeneity and the regressors, the bias is much more complex.

Stratified Partial Likelihood estimation allows the baseline hazard to differ across individuals in an arbitrary way. Coefficients are identified using within-individual variation. Whilst OLS estimates of wage premia have been augmented with fixed effects [e.g. Duncan and Holmlund, 1983, Villanueva, 2007] and duration models have used shared-frailty terms to capture scalar unobserved heterogeneity [van Ommeren et al., 2000], data limitations have prevented previous work from using this within-person variation in a stratified partial likelihood model. This approach is able to capture any heterogeneity that has the same shape within an individual across jobs and does not require proportionality of baseline hazards of different individuals. This means that unobserved heterogeneity could affect hazards differently at different points in the job spell. We capture these unobserved influences in a very flexible way, under the assumption that they are constant over the different jobs spells. This goes beyond the exiting literature.

One could argue that there are there are additional unobservables whose influence varies across spells generated by the same individual. Several of these potential determinants that are variable over spells, like the presence of children and business cycle effects are already incorporated in our model. We rely on exits from a job to identify willingness to pay, meaning that we do not capture the decisions of non-participating women who are prevented from entering the labour market by high commuting costs. Assuming that the willingness to pay is even larger in absolute terms for these women, we estimate lower bound (in absolute terms) for the willingness to pay of all women.

We use a proportional hazards specification of the form

$$\theta_{ij}(t|x) = \theta_j(t) \exp(\mathbf{X}_{ij}(t)'\beta)$$

for a worker j in job i with baseline hazard θ_j and (time-varying) covariate vector $\mathbf{X}_{ij}(t)$.

Estimating a Cox model in continuous time means that ties arise only as a consequence of imprecise measurement, not as a true feature of the data-generating process. To handle them, we use the Breslow approximation [Breslow, 1974, Peto, 1972]. It calculates the partial likelihood assuming that both individuals recorded to fail at the same time are in the risk sets at each other's failure times. This approximation introduces a bias of the coefficients towards zero, but it is the least computationally demanding and performs well if ties are not too frequent [Kalbfleisch and Prentice, 2002, p. 105].

Ridder and Tunalı [1999] argue that censoring is non-independent under what they call a synchronous observation plan, i.e. if analysis time returns to zero at the start of each new spell, as it does in our setup where we assume that the baseline hazard is defined in terms of the time elapsed in the current job. The problem could arise because the interaction between censoring at the end of the observation period and the timing of failure in an earlier spell within the same group affects risk sets. They provide an illuminating example of the bias this could cause [p. 211]. This illustrates the main limitation of the stratified approach, namely that like any fixed-effects method, it cannot accommodate heterogeneity that changes within individuals across jobs. To address this concern, we have also estimated the model on a sample where the censoring date is earlier in order to check for the sensitivity of our results with respect to censoring. This does not affect our main results.

Functional Form of Covariates The standard Cox model assumes a linear form for the log relative risk, but a number of diagnostic tools are available to determine whether this simple specification fits the data well. There is a clear trade-off in model choice here: Linear and log-linear relative risk specifications are tractable and produce estimates of marginal willingness to pay to reduce commuting distance that are easy to interpret and to compare to previous work. However, they may oversimplify a complex relationship. Since the goal of this analysis is estimating a marginal cost of commuting, we prioritise finding a well-fitting specification for the effects of the wage over other covariates (most of which are sets of binary variables anyway).

We explore fractional polynomials to find the best functional form for the wage. This method runs through a pre-determined set of functions, and applies a formal deviance criterion to choose the best form. The available functions are degree-1 and -2 additive combinations of natural logarithms, fractional and integer powers (hence the name) from the set $\{-2, -1, -0.5, 0, 0.5, 1, 2, 3\}$. Evaluation of alternative specifications uses comparative measures based on the log partial likelihood, such that a higher-degree functional form is adopted if it leads to a significant change in the transformed likelihood. For a detailed discussion of fractional polynomials including an application to a Cox model, see Royston and Altman [1994].

The log-transformation of the wage, which is attractive on theoretical grounds and much-used in the literature, is confirmed.[7] We let commuting distance enter the specification linearly to keep the estimate interpretable and comparable to previous studies. Dimensions of heterogeneity in willingness to pay, for example the measures of rental housing cost and childcare places, enter as dummies to ensure flexibility and produce willingness to pay estimates for interpretable groups.

Summary statistics are presented in table 1. On average, men's daily earnings are 32% higher than women's and their commutes are 20% longer. Over ninety percent of men, but only just over 60% of women, work full time and the share of jobs in unskilled occupations is higher among women than men. The appendix gives details on variable definitions, corrections applied and rules for inclusion in the sample. In addition, Table 2 compares summary statistics for childless women with those for all mothers and those for mothers of older children. Mothers earn lower daily wages and have shorter commutes than childless women and are less likely to be in full-time, skilled jobs.

Main Specification We specify a stratified partial likelihood model with a log relative risk that is linear in commuting distance and log-linear in the daily wage. This specification yields a marginal willingness to pay that depends linearly on the wage and is thus comparable to many previous estimates. Moreover, since time costs have previously been found to constitute a substantial part of overall commuting cost, it is theoretically plausible that they would vary in the opportunity cost of the worker's time, i.e. the wage rate. The hazard rate can be expressed as

$$\theta_i(t, \mathbf{X}) = \theta_i(t) \exp(\beta_w \ln(wage) + \beta_d distance + \beta_{\mathbf{x}} f(\mathbf{X}(\mathbf{t}))) \tag{2}$$

where the control vector $\mathbf{X}$ includes age as a time-varying variable and linear and quadratic terms for work experience up to eight years and a dummy for greater work experience, local unemployment rates and (sets of) dummies for full-time work, un-

[7] This analysis was done on a previous version of the estimation sample.

Table 1: Job-level summary stats, baseline estimation sample

	Women		Men	
	Mean	Std. Dev.	Mean	Std. Dev.
Daily Wage	57.29	27.65	75.82	31.78
Starting Wage	55.20	27.03	73.35	31.10
Final Wage	58.77	30.49	77.66	34.28
Distance (cond. < 100)	11.50	13.90	13.58	15.69
Age	35.46	10.24	34.80	9.990
Work experience	5.515	5.186	6.059	5.948
Full-time work	0.623	0.469	0.916	0.255
Unskilled job	0.114	0.309	0.0817	0.261
Core cities	0.281	0.450	0.271	0.444
Urban areas	0.499	0.500	0.500	0.500
Rural areas	0.220	0.414	0.229	0.420
Child(ren) present	0.396	0.487		
Child(ren) over age 12	0.190	0.385		
Observations	4,817,289		5,384,276	

Job-level values of time-varying variables are averages weighted by span length.

skilled occupation, regional categories of nationality, occupation, regional structure at the county level. Moreover, we include annual dummies to capture variation in the general macroeconomic conditions and national institutional environment, and regional GDP growth. We also include an interaction between each of the nine regional structure dummies and a dummy for zero distances, allowing a certain amount of discontinuity in willingness to pay. This addresses potential bias from different behaviours at the lower bound caused by different sizes of postcode areas in rural and urban areas. For women, we include a time-varying dummy switching to one at the time their first and second children are born. As this information is constructed from information on (mandatory) maternity leaves [Müller and Strauch, 2017], we are unable to reliably identify childbirth in men's biographies.

Predictability of Time-varying Covariates One of the advantages of the survival analysis methodology over more traditional regression models is its ability to model the

Table 2: Job-level summary stats, mothers and childless women

	Childless women		Mothers		Mothers of older children	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Daily Wage	60.00	28.05	53.32	26.57	54.13	26.74
Starting Wage	55.88	26.84	50.37	26.00	51.06	26.06
Final Wage	57.47	27.89	51.38	26.69	52.59	26.89
Distance	12.20	14.87	10.52	13.15	9.910	12.88
Age	32.08	10.25	41.22	7.786	45.61	5.278
Work experience	4.740	5.058	6.651	5.161	7.228	5.477
Full-time work	0.736	0.425	0.458	0.481	0.440	0.478
Unskilled job	0.107	0.300	0.126	0.321	0.146	0.341
Core cities	0.308	0.462	0.240	0.427	0.223	0.416
Urban areas	0.482	0.500	0.524	0.499	0.535	0.499
Rural areas	0.210	0.407	0.236	0.424	0.242	0.428
Child(ren) present	0	0	0.977	0.136	1.000	0.00862
Older children	0	0	0.468	0.484	0.924	0.203
Observations	1376874		939756		475669	

Job-level values of time-varying variables are averages weighted by span length. Mothers of older children are those whose youngest child is aged twelve or over. Presence of children is also time-varying, leading to values below one for the average of the children's variable. The mean distance is conditional on the distance being less than 100km.

influence of the current value of a time-varying covariate on the hazard. The level of observation in our data is a span, or national insurance record, which in principle allows for recording changes over time to any covariate, which helps with identification. For a stochastic covariate process to be valid in a survival model, it has to be predictable. Heuristically, a predictable process does not look into the future: At each point in time, a realisation of a predictable process is determined only by information on the past history of the process itself and its covariates, not their future paths.[8]

[8] For a more formal definition, recall that a stochastic process is a function that assigns random variables to time. It is predictable if and only if the preimage of this random variable is in the process's *filtration* [Kalbfleisch and Prentice, 2002, p. 157]. The filtration is usually defined as the sigma algebra generated by the history of the counting process of events up to and including the time t, as well as of the at-risk process and the covariate processes [Therneau and Grambsch, 2000, p. 18]. Left-continuity is a sufficient, but not a necessary condition for predictability. Therneau and Grambsch [2000, p. 5ff]

This condition could be problematic for the modelling of job mobility, if time-varying covariates are affected by agents' inside knowledge about the future path of the counting process. For example, a worker may learn about a job opportunity at a different firm and use that knowledge to negotiate his or her wage with her present employer. The employer may choose to raise the employee's wage to avoid losing her to a competitor. In this case, the covariate process – the wage – is affected by information on the future path of the job mobility process.

Therefore, its preimage is *not* contained in the filtration of the job mobility process up to the point where the wage negotiation takes place (it includes information on the counting process *after* this time), which makes the process non-predictable. In this case, the wage would not be a valid covariate in a model of job mobility. In our main study, we only use workers without a university degree, which should strongly reduce the impact of this problem, since individual wage negotiations are rare among this group.[9]

Estimation

Plugging the baseline functional form (2) into equation (1), marginal willingness to pay is given by

$$MWP = \frac{\beta_d}{\beta_w \cdot \frac{1}{w}} = \frac{\beta_d}{\beta_w} w$$

Main findings The baseline estimation (Table 3 implies a marginal commuting cost for childless women of 0.5% of the daily wage per kilometer of commuting distance (i.e., half this figure per kilometre travelled), or €0.31 at their mean wage. This is less than the €0.49 estimated for a mixed-gender sample of employees of Amsterdam's Vrije Universiteit by Russo et al. [2012], but more than the 0.4 Guilders or 18 Euro-cents estimated by Van Ommeren et al. [2000] at the mean wage for men, even accounting for inflation. This estimate, too, is for the Netherlands, so infrastructural, institutional and cultural differences might all contribute to a differences in marginal commuting cost. Also, it refers to the late 1980s, and it is plausible to assume that marginal commuting costs change over time, as prices for the monetary components of commuting cost, infrastructure, and the labour force composition change.

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provide a good intuitive explanation in their introduction, relating the concept of predictability to a game of chance.

[9] Hall and Krueger [2012] estimate a "dramatic" positive effect of education level on the probability of individual bargaining for the United States (p. 64).

Table 3: Baseline estimation: Partial likelihood model of exits from a job, stratified by individual.

	Women		Men	
Age	-0.0000112*	(0.00000485)	-0.0000493***	(0.00000479)
- Squared	-4.30e-09***	(1.55e-10)	-2.86e-09***	(1.61e-10)
Unskilled	0.0566***	(0.00532)	0.00980	(0.00507)
Full-time	0.142***	(0.00335)	0.180***	(0.00570)
Local growth	-0.212***	(0.0254)	0.124***	(0.0250)
Local unemployment	-0.000680	(0.000813)	0.000802	(0.000779)
Work experience	-0.264***	(0.00288)	-0.338***	(0.00301)
Squared	0.0384***	(0.000293)	0.0379***	(0.000299)
> 8 years	-0.978***	(0.0218)	-0.710***	(0.0209)
First child	-0.405***	(0.0321)		
Second child	0.401***	(0.0397)		
Older child(ren)	0.143***	(0.0354)		
Log wage	-0.615***	(0.00520)	-0.860***	(0.00443)
Distance	0.00318***	(0.000124)	0.00356***	(0.0000917)
Child × Wage	0.154***	(0.00784)		
Child × Distance	0.00157***	(0.000244)		
2nd child × Wage	-0.0661***	(0.00993)		
2nd child × Distance	-0.000249	(0.000329)		
Older child(ren) × Wage	-0.0104	(0.00888)		
Older child(ren) × Distance	-0.00105***	(0.000295)		
Observations	6,595,290		7,185,463	

Stratified Cox partial likelihood model, controls: age, unskilled occupation, full-time status, nationality, occupational field, motherhood (time-varying, for women), regional structure (9 types), local unemployment rate, local GDP growth. Zero distance in each region-type (urban to rural) captured by separate dummies). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Men's estimated marginal willingness to pay per kilometre is 20% less than that of childless women. However, due to men's higher wages, commuting cost per km at the respective mean wages are close together. This gap is very close to the one found by Le Barbanchon et al. [2019] using French administrative data on reservation wages and reservation commuting distances. Marginal commuting cost close to doubles upon the birth of a woman's first child. This increase supports the hypothesis that women's higher

Table 4: Marginal willingness to pay

No children	-0.00518	(0.000205)	-0.00414	0.000108
One child u12	-0.0103	(0.000510)	—	
One child over 12	-0.00785	(0.000552)	—	
2+ children, youngest u12	-0.00854	(0.000594)	—	
2+ children over 12	-0.00642	(0.000651)	—	

Estimated willingness to pay for men is for the whole sample of men, with and without children.

commuting costs are related to the time cost of non-market childcare work. We derive the information on childbirth from social security records and fathers are much less likely to interrupt their employment upon childbirth. Therefore, an analogous analysis for men which could shed additional light on gendered allocations of non-market work, is not possible with this dataset. Both an increased marginal effect of commuting distance on the job mobility hazard and a decreased marginal effect of the wage contribute to the increase in women's marginal commuting cost upon childbirth. In the context of our model, this implies that the marginal utility of a higher wage has decreased relative to that of a shorter commute. This is consistent with increased specialisation after the birth of the first child, with new mothers specialising in non-market work.

In addition to the impact on willingness to pay for a reduced commuting distance, family composition affects women's job mobility patterns directly. After a first birth, the hazard of a job-to-job transition declines, reflecting strong job protection legislation for new mothers, but this effect is reversed after a second child is born. The impact of the youngest child reaching the age of twelve (covariate "older child(ren)") on job mobility is also positive. This is consistent with our finding that willingness to pay decreases somewhat at this stage, although it does not go back to the level it was at before the first child's birth and possible related changes in the willingness to pay for other job attributes as children grow more independent.

Older and more experienced workers have more stable jobs, as we would expect, with a larger effect for men than for women. Women workers are less likely to leave a job, but men more likely to do so in areas of higher economic growth. Full-time workers of either gender are more likely to transition. This would be consistent with job mobility being associated with career progression. Unskilled jobs, identified using occupation codes, are less stable, potentially reflecting low job security or progression into more skilled work (recall that the effect of any time-invariant worker attributes will be captured by the individual-level baseline hazard).

For comparison, we estimate an unstratified model (table 5). The stratified model is our preferred specification: However, we provide this comparator to show that the differences are quantitatively important and to provide some evidence on the direction of bias introduced by unobserved heterogeneity, which a priori is unclear. The interaction of the baseline and the systematic component of the hazard in the Cox model is multiplicative. Therefore, unobserved heterogeneity leads to attenuation of covariate effects through dynamic sorting if observed and unobserved determinants of the hazard are uncorrelated [Ridder and Tunalı, 1999, and the references therein]. This is the case for the coefficient on the log wage in our case. However, failure to account for unobserved heterogeneity generates a substantial upward bias in the effect of commuting distance, many of the effects associated with children, as well as estimated marginal willingness to pay for both men and women.

This indicates that unobserved heterogeneity is correlated with motherhood, commuting distance and job-to-job mobility. Given that preferences over different types of jobs are likely to contribute to sorting into motherhood, this is not surprising. For example, part of the effect of the first birth on the transition hazard is explained by unobserved heterogeneity: Giving birth makes a job-to-job transition less likely, but women who give birth would also have "settled down" and had more stable job biographies in any case. Both the interaction effect of a child with the wage and with commuting distance are also overestimated when unobserved heterogeneity is unaccounted for. This substantiates our claim that stratification is a more appropriate technique of dealing with unobserved heterogeneity than a frailty method for this application. Note also that the gender gap in marginal commuting cost, at just under 18%, is slightly smaller in this specification compared compared to the main specification. This implies that the gender gap is not explained by person-constant unobserved heterogeneity in job-to-job mobility.

We also estimate a model on the sample censored in 2011, two years before the main sample, to see how sensitive our results are to the censoring pattern. This is regarded as a good test in order to investigate whether the assumption of independent censoring is appropriate (see Ridder and Tunalı [1999]). Coefficients in this specification are very similar to our main specification. The differences in estimated willingness to pay are small and insignificant.

We allow for interactions of the wage, distance, motherhood and part-time status in Table 9 and report the resulting separate marginal willingness to pay for part-time and full-time workers. Childless women's willingness to pay is virtually the same for full-time and part-time workers. However, the jump upon the birth of the first child is much smaller for part-time working mothers. This is consistent with them being less time-constrained than full-time working mothers, but the interaction between the wage, part-time status and the birth of the first child also suggests that whereas the wage becomes less important after the first birth for full-time working women, but not part-time working women. This suggests that part-time working mothers - perhaps due to lower wages overall - are less willing to give up (even more) pay. Another interesting result is that as their children get older, part-time working mothers' willingness to pay does not "bounce back" as is the case for full-time working mothers, but even increases by a small amount instead.

As discussed above, our main sample is restricted to vocationally educated women. We estimate one specification on the full sample, with dummies for university graduates (interacted with wage and distance) and top-coding interacted with the wage. It is not possible to calculate willingness to pay for top-coded women, since we do not have continuous pay information for them. But it is reassuring to note that the willingness pay for childless women estimated for non-top coded women on this extended sample is close to the baseline estimate, and the pattern of a large increase upon first birth persists (Table 9).

The availability of childcare places could be an important factor in constraining mothers' ability to accept longer commuting distances. The important role of childcare rationing and cost for female participation is well documented in the literature [Connelly, 1992, Del Boca, 2002, Del Boca and Vuri, 2007]. We use county-level information on the share of children under the age of three who attend daycare centres or are looked after by a qualified childminder (*Tagesmutter*). We argue that over the sample period, variation in this indicator is largely supply-driven. Daycare centres operated long waiting lists, indicating that demand far outstripped supply across the country. It is possible that areas with a large supply of childcare places have characteristics that also directly affect women's commuting cost - for example, if more affluent municipalities provide better transport infrastructure as well as more childcare places. However, the stratified method identifies differences in willingness to pay based on within-person changes over time (residential moves and childbirth), conditioning on time-invariant differences.

This information is only available for the period from 2005, restricting the sample relative to the baseline estimation. Estimating the baseline specification on this restricted sample leads to estimated willingnesses to pay which are not statistically different from our main estimates. We next allow willingness to pay for a reduced commuting distance to differ by childcare tercile for women who have young children (Table 7). There is a clear pattern: mothers in areas where more childcare is available have a lower willingness to pay to reduce commuting distance. Areas with more childcare places may also have implemented more other support for mothers that remains unobserved in our analysis. Therefore our results do not necessarily and directly produce a policy recommendation of providing more childcare places - however, they do provide evidence that motherhood gaps in willingness to pay are amenable to policy levers at the local level.

We estimate a model that allows willingness to pay by the area's settlement structure, distinguishing between core cities, densely populated areas and rural areas. There are a wide range of differences between those areas that could affect willingness to pay, in particular transport infrastructure. We report our estimates of willingness to pay in the different types of areas in Table 11. Differences in willingness to pay are quite small for childless women, with only a slightly lower willingness to pay in rural areas, driven by a lower marginal utility of the wage in rural areas, possibly related to a lower cost of living.

There is a much stronger contrast between mothers, with mothers in core cities having a much lower willingness to pay than mothers in either urban or rural areas outside of those core cities. The effect is driven by the interaction between the presence of a child, the regional structure and distance (rather than the wage), i.e. the marginal value of a shorter commuting distance increases by less after the first birth in core cities. A potential explanation would be differences in the availability of flexible working hours or workplace-based childcare: Core cities offer a greater variety of job bundles. This reduces the need to "triangulate" between home, work and childcare and makes it easier to find a good fit. In addition, core cities offer more hours of childcare. For example, a report by the Federal Statistical Office [Statistische Ämter des Bundes und der Länder, 2013] shows that the share of under-threes in full-time public childcare (defined as seven hours or more) in West Germany is currently highest in the cities of Frankfurt and Heidelberg. Even in these cities, the share was only slightly above 25%, highlighting the sparse provision of full-time childcare in West Germany.

The regional difference in willingness to pay persists as children get older. This could reflect better public transport in core cities, which allows older children to be more independent and rely less on parents to drive them. For men, only the difference between core cities and rural areas is statistically significant.

We are able to add information on housing costs to a subsample of the data (the period from 2005 to 2013). The confidence intervals for willingness to pay for this restricted sample overlap with the ones for the full sample for the baseline specification (table 13). We then estimate a model that allows for willingness to pay to differ between more and less expensive rental housing markets by interacting dummies for terciles of housing cost with the wage. Willingness to pay for commuting distance is slightly lower in more expensive areas, but the differences are very small.

The dataset contains some very large distances: The 99th percentile of distance for women is 314km. It seems improbable that individuals should travel this far every day. Instead, these are likely to be either weekend commuters or individuals who delay notifying their employer of their residential relocation. Therefore, we dropped all distances above 100 km in our main analyses. Including them in our linear specification leads to a substantial downward bias in estimated marginal willingness to pay, but if we allow for a kink at the 100km mark, the results again become similar to our baseline estimates (Table 15).

Endogeneity of residential location is a concern in our model. Workers may accept a job with a temporarily long commuting distance if they anticipate moving closer to their place of work in the future. To address this problem, we tested specifications which move all residential moves forward by one year, move them forward all the way to the beginning of the job spell during which they occurred, and which exclude all job spells that include a residential move between postcode areas. This last one is an imperfect solution: On the one hand, it also excludes some spells that do not violate exogeneity if women and men move for reasons other than to reduce their own commuting distance. On the other hand, an *ex-post* fixed residential location is not a sufficient condition for exogeneity. Nevertheless, these specifications excludes the previously described scenario, which would be the greatest cause for concern about biased estimates in this case. Results showed that marginal willingness to pay were similar to the baseline estimate, suggesting that the effect of endogenous residential moves is limited.

Conclusion

Our estimates on a large administrative dataset indicate that non-university educated women in Germany have a higher marginal willingness to pay to reduce commuting distance than men, which results in shorter observed commuting distances. Unobserved heterogeneity plays an important role, but gaps in the marginal willingness to pay by gender and motherhood remain after it is accounted for in a flexible way. Marginal willingness to pay is in the same range as previous estimates from search models for workers in other countries. We find evidence of a substantial motherhood gap in marginal commuting cost, consistent with gendered specialisation after first birth. The marginal willingness to pay of mothers of young children is somewhat reduced in areas where formal childcare is widespread, suggesting that the additional cost faced by these women can be mitigated using policy levers.

In additional analyses, we find that childless women have a similar marginal willingness to pay in core cities, urban and rural areas, but mothers in core cities have a much lower marginal willingness to pay than their counterparts in urban or rural areas. We also find than the increase in willingness to pay with first birth is smaller, but more persistent for part-time working mothers. Considering the housing market, willingness to pay is slightly lower in areas where housing costs are high, but the differences are small.

Differences in willingness to pay for job attributes such as commuting distance potentially play an important role in determining gender and motherhood wage gaps. Our measure of willingness to pay is a local one and can only approximate inframarginal differences such as the one between men's and women's average wages. Taking this local approximation at face value and extrapolating, our baseline specification suggests that mothers of young children would be willing to give up almost 12% of their daily wage to reduce their commuting distance from the sample mean to zero. In contrast, men would only be willing to give up less than five percent of their wage for a change of the same magnitude.

To put the gap in the marginal willingness to pay in the context of the gender pay gap, consider a man employed at the mean childless female wage and mean childless female commuting distance. To increase his commuting distance to the sample mean of men he would need to be compensated by a wage increase (as linear approximation) of 0.57%. This is about 2% of the gender wage gap for childless women. If the same calculation is done with respect to women with one child below 12 we can explain 3% of the gender wage gap with commuting preferences.

Note that, similarly to a Blinder-Oaxaca composition, the choice of endowment matters: If we use the mean wage and commuting distance of men and weight it by the preferences of women then we get an explained part of 3% gender wage gap for childless women and 10% explained gender wage gap for women with children below 12 years. In other words, a person with the preferences of a mother of a child under twelve who was currently employed at men's average wage and commuting distance would be willing to give up 10% of the gap to lower their commuting distance to mothers' average distance. The higher value is due to men's higher wages and commuting distances. Thus, our empirical results in combination with this back-of-the-envelope calculations indicate that commuting preferences indeed contribute to gender wage gap and in particular motherhood wage gap, but only to a limited extent.

Table 5: Early censoring and non-stratified specification

	Censored in 2011		Unstratified Model	
	Women	Men	Women	Men
Log wage	-0.741***	-0.794***	-0.491***	-0.630***
	(0.00658)	(0.00515)	(0.00205)	(0.00185)
Distance	0.00407***	0.00358***	0.00345***	0.00372***
	(0.000153)	(0.000106)	(0.0000609)	(0.0000476)
Age	-0.000209***	-0.000161***	0.0000501***	0.000126***
	(0.00000682)	(0.00000593)	(0.00000166)	(0.00000167)
Squared	-3.38e-09***	8.52e-10***	-2.51e-09***	-4.23e-09***
	(2.19e-10)	(2.01e-10)	(6.02e-11)	(6.13e-11)
Work experience	-0.267***	-0.345***	-0.512***	-0.595***
	(0.00363)	(0.00350)	(0.00163)	(0.00165)
Squared	0.0403***	0.0394***	0.0544***	0.0603***
	(0.000355)	(0.000339)	(0.000196)	(0.000200)
Full-time	0.203***	0.179***	0.191***	0.212***
	(0.00439)	(0.00697)	(0.00161)	(0.00299)
First child	-0.526***		-0.768***	
	(0.0414)		(0.0147)	
Second child	0.588***		0.0214	
	(0.0532)		(0.0171)	
Older child(ren)	0.431***		-0.0643***	
	(0.0474)		(0.0160)	
Child × Wage	0.182***		0.192***	
	(0.0101)		(0.00367)	
Child × Distance	0.00183***		0.00204***	
	(0.000311)		(0.000131)	
2nd child × Wage	-0.117***		-0.00179	
	(0.0133)		(0.00433)	
2nd child × Distance	0.000534		-0.000433**	
	(0.000430)		(0.000168)	
Older child(ren) × Wage	-0.0960***		0.00466	
	(0.0119)		(0.00405)	
Older child(ren) × Distance	-0.00103**		-0.00162***	
	(0.000385)		(0.000155)	
Observations	5360802	5848739	6595290	7185463

Table 6: Marginal willingness to pay with early censoring and non-stratified specification

No children	-0.00549	(0.00021)	-0.00702	(0.000126)
One child u12	-0.0105	(0.000540)	-0.0184	(0.000439)
One child over 12	-0.00743	(0.000518)	-0.0131	(0.000441)
2+ children, youngest u12	-0.00951	(0.000611)	-0.0168	(0.000549)
2+ children over 12	-0.00700	(0.000599)	-0.0116	(0.000592)
Men	-0.00451	(0.000136)	-0.00591	(0.0000769)

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. (3) and (4) include a full set of year dummies.

Table 7: Estimation by availability of childcare in the local area

	Childcare Sample		Full model	
Log wage	-0.639***	(0.00749)	-0.594***	(0.00982)
Distance	0.00322***	(0.000169)	0.00318***	(0.000241)
Age	0.000119***	(0.00000949)	-0.0000298**	(0.0000103)
Squared	-6.85e-09***	(2.89e-10)	-5.24e-09***	(3.09e-10)
Work experience	-0.226***	(0.00442)	-0.255***	(0.00468)
Squared	0.0337***	(0.000411)	0.0352***	(0.000429)
Full time	0.129***	(0.00451)	0.127***	(0.00465)
First child	-0.345***	(0.0466)	0.0571	(0.108)
Second child	0.435***	(0.0565)	0.497***	(0.0586)
Older child(ren)	-0.134**	(0.0500)	-0.103	(0.117)
Child × Wage	0.152***	(0.0114)	-0.175***	(0.0263)
Child × Distance	0.00151***	(0.000340)	0.00438***	(0.000873)
2nd child × Wage	-0.0484***	(0.0141)	-0.0482***	(0.0146)
2nd child × Distance	-0.000531	(0.000444)	-0.000495	(0.000457)
Older child(ren) × Wage	0.0375**	(0.0126)	0.376***	(0.0286)
Older child(ren) × Distance	-0.00134***	(0.000399)	-0.00441***	(0.000927)
Childcare Tercile: 2nd			0.434***	(0.0353)
3rd			0.749***	(0.0416)
2nd × Young child			-0.0788	(0.114)
3rd × Young child			-0.116	(0.118)
2nd × Young child × Dist			-0.00258**	(0.000925)
3rd× Young child × Dist			-0.00404***	(0.000962)
2nd × Young child × Wage			0.280***	(0.0279)
3rd × Young child × Wage			0.277***	(0.0289)
Observations	3622355		3501774	

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 8: Willingness to pay by availability of childcare in the local area

Baseline: No children	-0.00505	(0.000269)	-0.00535	(0.000412)
One child u12	-0.00972	(0.000674)	-0.0121	(0.00117)
One child over 12	-0.00754	(0.000770)	-0.00803	(0.00102)
2+ children, youngest u12	-0.00784	(0.000798)	-0.00980	(0.00124)
2+ children over 12	-0.00574	0.000936)	-0.00602	(0.00116)
Medium: One child u12			-0.0102	(0.000941)
2+ children, youngest u12			-0.00836	(0.000992)
High: One child u12			-0.00716	(0.000963)
2+ children, youngest u12			-0.00561	(0.000988)

Baseline MWP for mothers of young children in second model is for the lowest tercile of childcare.

Table 9: Part-time workers and graduates

	Part-time		Incl. Graduates	
Log wage	-0.634***	(0.00594)	-0.591***	(0.00479)
Distance	0.00330***	(0.000137)	0.00319***	(0.000117)
Age	-0.00000464	(0.00000487)	-0.00000668	(0.00000455)
Squared	-4.43e-09***	(1.55e-10)	-4.54e-09***	(1.46e-10)
Work experience	-0.266***	(0.00288)	-0.262***	(0.00265)
Squared	0.0385***	(0.000293)	0.0382***	(0.000271)
Full time			0.140***	(0.00308)
First child	-0.468***	(0.0402)	-0.392***	(0.0282)
Second child	0.387***	(0.0398)	0.443***	(0.0360)
Older child(ren)	0.123*	(0.0500)	0.129***	(0.0329)
First child × Wage	0.182***	(0.00967)	0.150***	(0.00676)
First child × Distance	0.00201***	(0.000302)	0.00148***	(0.000217)
Older child(ren) × Wage	-0.0281*	(0.0122)	-0.00605	(0.00818)
Older child(ren) × Distance	-0.00163***	(0.000402)	-0.00102***	(0.000275)
2nd child × Wage	-0.0598***	(0.00996)	-0.0851***	(0.00887)
2nd child × Distance	-0.000122	(0.000330)	-0.000401	(0.000301)
Part time	-0.490***	(0.0356)		
PT × Child	0.591***	(0.0541)		
PT × Older child(ren)	-0.393***	(0.0641)		
PT × Wage	0.101***	(0.00916)		
PT × Distance	-0.000418	(0.000256)		
PT × Wage × Child	-0.189***	(0.0139)		
PT × Distance × Child	-0.00102*	(0.000444)		
PT× Wage × Older child(ren)	0.146***	(0.0162)		
PT × Dist × Older child(ren)	0.00150**	(0.000555)		
Graduate			0.212***	(0.0354)
Graduate × Wage			-0.0574***	(0.00807)
Graduate × Distance			-0.0000843	(0.000222)
Top-coded × Wage			-0.107***	(0.00238)
Observations	6595319		7811505	

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 10: Willingness to pay for part-time workers and graduates

No children	-0.00520	(0.000220)	-0.00540	(0.000202)
One child u12	-0.0117	(0.000651)	-0.0106	(0.000485)
One child over 12	-0.00766	(0.000701)	-0.00819	(0.000553)
2+ children, youngest u12	-0.0101	(0.000729)	-0.00812	(0.000549)
2+ children over 12	-0.00659	(0.000762	-0.00612	(0.000614)
Part time: No children	-0.00541	(0.000444)		
One child u12	-0.00716	(0.000569)		
One child over 12	-0.00884	(0.000829)		
2+ children, youngest u12	-0.00625	(0.000596)		
2+ children, all over 12	-0.00749	(0.000869)		

Table 11: Estimation by region type

	Women		Men	
Full time	0.193***	(0.00366)	0.179***	(0.00570)
First child	-0.519***	(0.0348)		
Second child	0.447***	(0.0437)		
Older child(ren)	0.384***	(0.0391)		
Log wage	-0.780***	(0.00783)	-0.812***	(0.00696)
Distance	0.00402***	(0.000250)	0.00344***	(0.000173)
Child × Wage	0.168***	(0.00872)		
Child × Distance	0.000883	(0.000531)		
2nd child × Wage	-0.1000***	(0.0110)		
2nd child × Distance	0.000135	(0.000359)		
Older child(ren) × Wage	-0.0799***	(0.0101)		
Older child(ren) × Distance	-0.00218**	(0.000730)		
Urban × Wage	0.0170*	(0.00848)	-0.0857***	(0.00840)
Rural × Wage	0.0681***	(0.0107)	-0.0303**	(0.0110)
Urban × Distance	0.000247	(0.000314)	0.000301	(0.000217)
Rural × Distance	-0.000683	(0.000357)	-0.000114	(0.000251)
Urban × Child × Wage	-0.00270	(0.00320)		
Rural × Child × Wage	-0.00404	(0.00404)		
Urban × Child × Distance	0.00145*	(0.000625)		
Rural × Child × Distance	0.00194**	(0.000688)		
Observations	6581025		7185463	

Table 12: Marginal willingness to pay by region type

Cities: Childless women/all men	-0.00516	(0.000323)	-0.00423	(0.000215)
One child u12	-0.00802	(0.000799)		
One child over 12	-0.00395	(0.000864)		
2+ children, youngest u12	-0.00709	(0.000769)		
2+ children over 12	-0.00362	(0.000832)		
Densely pop.: Childless women/all men	-0.00560	(0.000256)	-0.00416	(0.000150)
One child u12	-0.0111	(0.000567)		
One child over 12	-0.00758	(0.000560)		
2+ children, youngest u12	-0.00967	(0.000584)		
2+ children, all over 12	-0.00678	(0.000590)		
Rural: Childless women/all men	-0.00469	(0.000361)	-0.00394	(0.000219)
One child u12	-0.0113	(0.000795)		
One child over 12	-0.00878	(0.000804)		
2+ children, youngest u12	-0.00974	(0.000755)		
2+ children, all over 12	-0.00775	(0.000773)		

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Region-distance/wage interactions are allowed to differ for older children, but effects are insignificant.

Table 13: Willingness to pay by housing cost

	Baseline		Full model	
Age	0.0000429***	(0.00000817)	0.0000484***	(0.00000818)
Squared	-6.78e-09***	(2.51e-10)	-6.73e-09***	(2.52e-10)
Full time	0.125***	(0.00422)	0.126***	(0.00422)
Work experience	-0.208***	(0.00398)	-0.209***	(0.00398)
Squared	0.0332***	(0.000378)	0.0333***	(0.000378)
First child	-0.349***	(0.0428)	-0.416***	(0.0617)
Second child	0.438***	(0.0520)	0.442***	(0.0521)
Older child(ren)	-0.0554	(0.0461)	-0.0506	(0.0462)
Log wage	-0.621***	(0.00693)	-0.641***	(0.0107)
Distance	0.00316***	(0.000158)	0.00325***	(0.000158)
Child × Wage	0.155***	(0.0105)	0.177***	(0.0153)
Child × Distance	0.00139***	(0.000315)	0.00133***	(0.000316)
2nd child × Wage	-0.0564***	(0.0130)	-0.0577***	(0.0130)
2nd child × Distance	-0.000450	(0.000414)	-0.000433	(0.000414)
Older child(ren) × Wage	0.0209	(0.0116)	0.0195	(0.0116)
Older child(ren) × Distance	-0.000984**	(0.000372)	-0.000987**	(0.000372)
Rent Tercile: 2nd			-0.101	(0.0522)
3rd tercile			0.194***	(0.0580)
2nd tercile × Child			0.109	(0.0724)
3rd tercile × Child			0.0506	(0.0806)
2nd tercile × Wage			0.0425***	(0.0127)
3rd tercile × Wage			0.00422	(0.0139)
2nd × Child × Wage			-0.0322	(0.0180)
3rd × Child × Wage			-0.0247	(0.0198)
Observations	4118548		4118548	

Table 14: Marginal Willingness to Pay by Housing Cost

No children	-0.00509	(0.000258)	-0.00507	(0.000258)
One child u12	-0.00975	(0.000650)	-0.00986	(0.000675)
One child over 12	-0.00800	(0.000730)	-0.00807	(0.000744)
2+ children, youngest u12	-0.00784	(0.000760)	-0.00794	(0.000770)
2+ children over 12	-0.00620	(0.000867)	-0.00628	(0.000871)
Medium: No children			-0.00542	(0.000276)
One child u12			-0.0101	(0.000692)
One child over 12			-0.00827	(0.000765)
2+ children, youngest u12			-0.00810	(0.000788)
2+ children, all over 12			-0.00642	(0.000892)
High: No children			-0.00510	(0.000260)
One child u12			-0.00944	(0.000649)
One child over 12			-0.00772	(0.000715)
2+ children, youngest u12			-0.00764	(0.000744)
2+ children, all over 12			-0.00604	(0.000840)

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 15: Long distances

Age	-0.0000113*	(0.00000469)
Squared	-4.18e-09***	(1.50e-10)
Unskilled	0.0560***	(0.00514)
Full time	0.139***	(0.00324)
Work experience	-0.265***	(0.00278)
Squared	0.0383***	(0.000284)
> 8 years	-0.978***	(0.0214)
First child	-0.373***	(0.0309)
Second child	0.401***	(0.0387)
Older child(ren)	0.138***	(0.0344)
Log wage	-0.606***	(0.00496)
Distance	0.00277***	(0.000118)
Child × Wage	0.145***	(0.00755)
Child × Distance	0.00169***	(0.000238)
2nd child × Wage	-0.0663***	(0.00966)
2nd child × Distance	-0.000246	(0.000321)
Older child(ren) × Wage	-0.00840	(0.00862)
Older child(ren) × Distance	-0.00109***	(0.000288)
Distance× Long distance	-0.00201***	(0.000115)
Distance× Long distance × Child	-0.00154***	(0.000235)
Distance× Long distance × Second Child	0.000356	(0.000317)
Distance× Long distance × Older child(ren)	0.00107***	(0.000285)
Observations	6836871	
Marginal Willingness to Pay		
No children	-0.00457	(0.000198)
One child u12	-0.00967	(0.000495)
One child over 12	-0.00717	(0.000538)
2+ children, youngest u12	-0.00799	(0.000578)
2+ children over 12	-0.00583	(0.000635)

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix

Structure of the dataset

The data consists of spans that can last for any period between a day and a year. A span ends when an event mandates an employer notification to the national insurance agency. These events include the beginning and end of a job spell, but also any change that triggers a change in contribution liabilities, such as a change in the wage. Exits into maternity leave are identifiable separately from other exits. In principle, changes to the worker's home address could also be recorded immediately, in practice this is likely to happen with some delay. If no even happens within a year - i.e. if an employee works for the same employer at the same wage in the same position during the whole of a calendar year - then the employer still needs to make a notification for the year.

Variable Definitions

Commuting distance The distance is the Euclidean distance between the centroid of the employee's residential and work post code. Work post codes are measured at the plant level.

Earnings and hours of work The dataset records daily earnings and a discrete variable with the categories full-time, "large" part-time and "small" part-time. There is no more precise information on hours worked and it is therefore not possible to construct an hourly wage.

Occupational Characteristics The dataset contains two **occupation** variables, a 3-digit variable based on the 1988 classification [Bundesanstalt für Arbeit, 1988], and a 5-digit variable based on the most recent classification [Bundesagentur für Arbeit, 2011]. Since the observation window ends in 2010 and re-coding of older observations to the 2010 system is not error-free, the older variable is likely to be more accurate. However, the 2010 classification combines a horizontal (occupation) and a vertical (skill level) dimension. I recover the skill level information, which is absent from the older variable at the available aggregation level. For the horizontal occupation information, I match the 1988 information to 53 task-based *occupational fields*, as defined by the Institute for Vocational Education and Training.

Unskilled is the lowest of four skill levels, characterised as a *un- or semiskilled activity* with simple or routine tasks of little complexity, where formal vocational training not usually required.

Regional Characteristics We match the individual data to classification which distinguishes 9 **types of district**, due to the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR). They are based on administrative districts, but differ from them where administrative divisions group structurally different areas into one unit. We include dummies for the type of area and interactions with a dummy for zero distances (workers who live and work in the same postcode area) to account for the larger geographical size of postcode areas in sparsely populated regions.. Moreover, we estimate a separate willingness to pay for three broader types of area, proposed by the institute as characterising city-periphery relationships:

- Core cities
- Districts with a predominantly urban character ("urban areas")
- Districts with a predominantly rural character and rural areas ("rural areas")

For more detailed information on the classification, see Görmar and Irmen [1991] or the institute's online information [Bundesinstitut für Bau-, Stadt- und Raumforschung, 2006].

Rental cost The proxy for rental cost is also provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development. It is based on ask prices for flats gathered from online platforms and newspapers, using the following criteria:

- pure rental prices with no heating or other utilities included
- non-furnished flats between 40 and 130 square metres
- the ad is displayed for no more than six months
- some additional filters to exclude implausible levels and changes

The providers suggest that their measure is likely to omit some flats offered by very large housing companies, particularly in Berlin and Hamburg, who use their own information channels. It is also likely to omit some flats in rural areas which are only advertised on local notice boards or find a new tenant by word of mouth. Actual rent paid may be slightly lower in areas of low demand where prospective tenants are able to negotiate a lower price.

Treatment of Missings Individuals' **Nationality**, while not necessary constant, is unlikely to change back and forth, more so in a short time span. Therefore, up to two subsequent missing values were filled if coded nationality was identical for the individual before and after the gap arising due to the missing values. This affects a small part of the sample, since less than 1 percent of observations originally had nationality coded as missing.

If the **wage** was coded as zero or missing in a subspell, but valid wage information was available in another spell within the same aggregate job spell (a continuous employment at the same firm), the valid wage information was extended to the missing observation.

Residential Moves A typical job spell used in the model consists of a number of spans, which correspond to national insurance records. There is at least one record per year, plus additional records in case of changes in the employee's data, e.g. a change in the wage. The residential location corresponds to the end of the sub-spell. It is not known to what extent employers proactively register their employees' changes in residential location with national insurance. It is plausible that at least some employers simply wait until the next regular entry is due, so we might observe residential moves with a certain delay.

Children We use two dummies for the birth of the first and second child, respectively. These dummies stay at one forever. We additionally use dummies to capture the youngest child in the family reaching the age of twelve, to reflect differences in the time constraints of parenting younger versus older children.

Sample Construction

Employment spells according to the above-mentioned definition are included in the sample if:

- they are not overlapped by a spell in registered unemployment or an active labour market programme [10], a mobility-related subsidy or retirement. Small overlaps of up to three days are tolerated. Individuals are observed as registered unemployed if they are eligible for top-up unemployment benefits to close the gap between low earnings and the subsistence level. In this case, the wage paid by the employer is not the wage actually perceived by the worker, who faces a wage distribution that is truncated at the legal minimum subsistence level. A similar distortion of the wage-commuting trade-off arises in the case of a mobility subsidy. Participants in active labour market programmes, on the other hand, do not choose their place of work, and their behaviour can therefore not be adequately reflected in the model. Therefore, these cases are not included in the sample. Selection into standard (i.e. non-subsidised) employment is not addressed here.

- the individual is never recorded as having a university degree, with certain corrections applied. To avoid complications arising from the decision to return to education, we do not include employment spells before university graduation. We do not know if individuals acquire a university degree after the end of the observation window. Eight to ten years after leaving vocational education, this is unlikely to apply to many individuals.

- they belong to a job identified as the main job at that time (more details below).

- they are not part of a seasonal work pattern, i.e. the worker does not return to the same employer without an intervening spell at a different firm. Spells with the same employer with gaps of up to a week are considered part of a single job to avoid misinterpreting administrative delays to contract renewal as seasonal work. About a quarter of spells are dropped for this reason.

- they are part of the inflow sample starting on January 1st, 2000. The data is right-censored on December 31st, 2013.

- they last for more than 60 days. Temporary workers whose contracts last less than two months are usually not liable to pay full social security contributions, which should preclude their inclusion in the sample. Spells of under two months could be due to exceptions in the national insurance treatment, early firings, miscoded part-time work, or misreported dates, which are difficult to disentangle. Moreover, the optimisation process underlying short-term job location may differ substantially

[10] a programme to support the long-term unemployed, publicly sponsored employment, or a seasonal or temporal work placement organised by the employment agency

from the one related to long-term job mobility decisions and temporary residential relocations are likely to not appear in the data, which makes distance calculations unreliable. Therefore, spells of under two months are dropped.

- the implied monthly wage is within the limits that make a worker liable to pay national insurance contributions (*Geringfügigkeitsgrenze* and *Beitragsbemessungsgrenze*). Due to different timings of reports, wage information in some spells which are not actually subject to contributions was included in the original dataset.

These criteria are applied in the stated order, e.g., if short and long job spells form a seasonal work pattern, they are dropped, even if the short spells would later be excluded by the two-months rule.

We exclude spells where either the place of work or the place of residence was missing or invalid, or where an individual was recorded as living in, or a firm recorded as being located in, two or more different zip code areas at once, since no valid commuting distance could be determined.

Treatment of overlapping employment spells Overlapping spells present a challenge to the model, since neither the theory nor the empirical model allows for an agent to be in two states at once. To keep the model tractable, we make the simplifying assumption that individuals have one main job, and mobility behaviour in any other jobs is not reflected in the model. Cases where no clear hierarchy of parallel jobs can be determined are excluded.

Multiple job spells with different employers at the same time Overlapping job spells of the same individual with different employers are excluded, except in the following cases:

- **Transitional overlap:** If the overlap is less than two weeks, both spells are included, with the transition assumed to occur at the start of the overlapping period.
- **Short temporary jobs:** If one and only one of the jobs lasts for less than a year and the other one is at least three times as long, the longer spell is considered the main job and included in the sample.

- **Part-time jobs:** If one of the jobs is full-time whereas the other one is part-time, the full-time spell is considered the main job and included in the sample

The three criteria are hierarchical, i.e. we first check for transitional overlap, then for temporary jobs, then for part-time jobs.

Multiple spans with the same employer Spans are records, i.e. within-job observations. In the case of overlap between multiple spans, the outcome - job mobility - is unaffected, and the only question is which values of time-varying covariates are valid at which point in time. Pairs of these spells were split. The span created from the overlapping spans has the covariates of the two original spans if they are non-contradictory. Otherwise, the covariate is set to missing. In the case of conflicting wage information, if the difference is less than 5%, the mean is used. [11]

[11] Browsing the data where spells overlap suggest that while some probably refer to changing wages, others appear to refer to bonuses instead, which would imply that the true wage is the sum of both recorded wages. Separating the two cases would involve (more) arbitrary cut-offs. Since less than 1 % of spells are affected, so no attempt at this is made. In the rare case of triple or greater multiple overlaps which only affects about 1 in 2000 spells, the overlapping portions were dropped without any corrections to the covariates.

2 Taking Back Control? Trading Off Wages And Schedule Autonomy

with Gerard van den Berg

Statement on co-authorship: This chapter is joint work with Gerard van den Berg. I have made major contributions to all aspects of the work, including data cleaning, development of the economic frameworks used in the paper, estimation, placement in the literature and writing up. In addition to offering guidance on all aspects of the work in his role as PhD supervisor, Gerard van den Berg contributed to the development of the research questions and empirical specification.

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Working schedule autonomy is frequently named as an important characteristic of family-friendly workplaces and a means of improving labour market opportunities for workers with care-giving responsibilities, especially women [e.g. Parker and Wang, 2013, Equality and Human Rights Commission, 2017]. More formally, in a Beckerian framework where time inputs and market-bought goods are inputs into home production, workers gain utility from schedule autonomy depending on the production functions and marginal utilities for commodities other than market work. People for whose home production some types of time are much more productive than others will benefit most from being able to reallocate those time inputs away from market work. Examples of such commodities are picking up and dropping off children and home-cooked meals. However, Mas and Pallais [2020] argue that in real workplaces, flexible schedules come bundled with less attractive job attributes and in fact are detrimental to combining market work and care giving responsibilities. In this paper, we use novel data to examine whether, in the institutional setting of a social market economy, the earlier argument that flexibility is valued by those with caregiving responsibilities and traded off against wages, still holds.

Analyses of willingness to pay, such as ours, quantify the trade-offs between increased home production enabled by schedule autonomy and potentially lower wages in the market, a potentially important contributor to gender and parenthood wage gaps. Several studies have analysed schedule autonomy, its determinants and its effects on worker satisfaction in Germany. We will briefly review this literature here and subsequently discuss recent work on willingness to pay for schedule autonomy from the US.

Definitions of what exactly schedule autonomy is vary, as do findings about its determinants and impacts. Hunt [2013] defines flexibility as overtime that is compensated by time-in-lieu. Using the German SOEP, she finds that workers who access this type of flexible schedule are slightly *less* satisfied with their work, but more satisfied with the amount of leisure time they have. She uses the cyclicality of overtime to disentangle worker- from employer-led schedules. With this approach, measuring autonomy over short-term scheduling decisions, for example how to allocate working hours across a single day or week, remains challenging. However, productivity of time inputs into home production is likely to vary across these short time horizons - with peaks for the school run and mealtimes - as well as longer periods, when it is driven by household composition and child age.

Another option is to directly use worker-reported schedule autonomy. Hanglberger [2010] finds that complete schedule autonomy increases job satisfaction of full-time employees, but not limited autonomy through a flexitime scheme with a working time account, which usually includes some core period with mandatory attendance. The analysis accounts for unobserved individual heterogeneity through fixed effects. Since the analysis excludes the self-employed, the group of workers with complete schedule autonomy is a fairly small one, at less than 15% of employees (own calculation). He finds no effect of the timing of non-autonomously scheduled hours (standard working hours vs weekend or evening work) and no significant interactions with the presence of children in the household.

Wanger [2017] considers effects of schedule autonomy and a range of personal and job characteristics on both satisfaction with working time and job satisfaction more widely defined. Her measure of schedule autonomy is based on the survey question, "How often do you succeed in taking your private interests and the interests of your family into account when planning your working hours?". It therefore focuses less on the formal arrangement in place and more on de facto incidence of work-family scheduling conflicts. This also means that measured autonomy may depend on the demand for it, as those workers whose family and personal life places fewer demands on their time may report greater schedule autonomy.

She finds that autonomy over schedules and work planning increases satisfaction with working time as well as with one's job in general. Unlike general job satisfaction, working time satisfaction is not related to personal characteristics. Although she excludes this variable from her main model due to concerns about reverse causality, she mentions that working time satisfaction is affected by satisfaction with the wage, opening up interesting questions of wage-amenity trade-offs which we explore in this paper. The earned wage itself also has a small positive effect on both satisfaction measures.

Zapf and Weber [2017] study the determinants, rather than the effects of schedule autonomy using a version of SOEP that includes a linked employer survey, the SOEP-LEE, and focusing once more on the working time arrangement itself. They find that worker characteristics have little impact on observed schedule autonomy, which is instead largely determined by employer and job characteristics. Economic theory suggests that the benefits workers obtain from schedule autonomy will be heterogeneous. However, whether those workers with the greatest utility gain from schedule autonomy actually consume more of it will depend on search frictions, the offer distributions faced by different workers and income effects. We are therefore interested in studying willingness to pay directly, with a particular focus on gender and parenthood.

The literature has previously found that schedule autonomy leads to different outcomes for men and women. Lott and Chung [2016] use data from the German SOEP to show that for men, schedule autonomy is associated with increases in income and overtime. Whilst women in full-time jobs with schedule autonomy also work more overtime, their income does not increase.

The question of schedule autonomy in a context of a wage-amenity trade-off has received less attention in the literature on the German labour market. Heywood et al. [2007] use the British 1998 Workplace Employee Relations Survey, a linked employer-employee survey and find large compensating wage differentials of 20% for family-friendly practices, with the effect mainly driven by schedule autonomy. A dummy variable for schedule autonomy is generated, identifying cases where both workers and managers report that flexible hours are available. They then instrument for workplace practices with employer (in practice, manager) attitudes. However, "progressive" manager attitudes may affect a range of workplace characteristics and even be considered an amenity in itself. It is therefore unclear whether this estimation strategy succeeds in isolating a premium for schedule autonomy.

Experimental evidence of willingness to pay for schedule autonomy of telephone interviewers in the United States is provided by Mas and Pallais [2017]. They find that most workers are not willing to pay for schedule autonomy and that their aversion to evening and weekend work drives their dislike of employer-led flexibility that introduces unpredictability for workers. However, there is a minority of workers who have a high willingness to pay for schedule autonomy, which could sustain pay differentials.

We provide new, direct evidence on willingness to pay for schedule autonomy of workers in Germany. By using data on reservation wages themselves, we show that the hedonic wage premium is large and wrong-signed as an indicator of worker willingness to pay. We confirm previous findings of substantial heterogeneity across workers in the experimental and non-experimental literature in Europe and the US. We also show that beyond the job offer acceptance/rejection decision, the trade-off between wages and schedule autonomy matters at other points of the utility function, such as for the determination of a fair wage, linking the literature on job attributes and search to the one one fair wages.

In the following subsection, we set out a theoretical framework for schedule autonomy in a search environment and motivate our measure of willingness to pay. In subsection 2.2, we describe the new survey data we use. subsection 2.3 quantifies willingness to pay for schedule autonomy, carefully distinguishing worker- from employer-led flexibility and examine worker heterogeneity along several dimensions. We test a theoretical prediction that external factors such as the presence of children in the household and access to support networks determine willingness to pay for schedule autonomy. Furthermore, we analyse the role of schedule autonomy through the lens of the fair wage-effort hypothesis of Akerlof and Yellen [1990] and test some of its predictions using our sample as well as the main SOEP panel.

2.1 Willingness to Pay for Schedule Autonomy in a Search Model

If workers engage in household production as well as market work and need to allocate time inputs amongst those competing uses, schedule autonomy always enables a weakly better allocation of time inputs. Workers could thus achieve greater overall utility from consumption of household and market goods. The utility workers gain from a job thus depends on the schedule autonomy it grants, as well as its wage. Gronberg and Reed [1994] present a search model with job attributes, in which the reservation wages for "good" and "bad" jobs will differ from each other. Their framework has subsequently been applied to estimate workers' willingness to pay for job attributes such as commuting distance [van Ommeren et al., 2000].

We apply their extension of the classic Burdett [1978] model to schedule autonomy. Consider an employed worker in a job with wage w and schedule type x. In most of our empirical application, x is a binary variable which takes the value 0 for jobs with a fixed schedule and the value 1 for a worker-led flexible schedule. We abstract from labour supply issues. The worker receives alternative job offers (w^*, x^*) both while employed and while unemployed in a Poisson process with parameter λ.

The expected discounted stream of utility from accepting job offer (w, x) over the whole of the life course is

$$\rho R(w,x) = u(w,x) + \lambda \int\int \max\{0, R(w^*,x^*) - R(w,x)\} dF(w^*,x^*) \\ + \delta(U - R(w,x))$$

where ρ is a discount parameter and U is the expected present value of unemployment. Lifetime utility is thus composed of an instantaneous component, a continuation value in case of a job switch and another continuation value in case of exogenous job loss.

The optimal strategy, as in the one-dimensional job case treated by Mortensen [1986], is myopic. The worker pursues a reservation utility strategy: She accepts all job offers which offer a higher instantaneous utility than her present job if she is employed or than unemployment if she is unemployed, since the "stream" of jobs that will become available in the future is not affected by the current state. In the binary case we consider here, this generates a pair of reservation wages $(\bar{w}_0, \bar{w}_1)$ for autonomous and non-autonomous jobs which are defined by the condition

$$u(\bar{w}_1, 1) = u(\bar{w}_0, 0) = u(w, x)$$

if the worker is currently employed at wage w, or else by the condition

$$u(\bar{w}_1, 1) = u(\bar{w}_0, 0) = U$$

if the worker is currently unemployed.

Then, $\bar{w}_0 - \bar{w}_1$ is a measure of willingness to pay for schedule autonomy: A wage increase of this magnitude would exactly compensate a worker for the loss of schedule autonomy.

In measuring willingness to pay for schedule autonomy, it is crucial to distinguish worker-led from employer-led working time arrangements, even if both frequently come under the umbrella term “flexible working hours”. If flexibility means the option for workers to optimally allocate a fixed number of working hours across time, it will always be weakly beneficial. However, if fixed hours are replaced by a defined output which will take an a priori uncertain amount of time to produce, working hours could be unpredictable even without employers having the right to directly vary them. This would expose workers to an additional risk of lost home production and these arrangements would therefore be particularly unattractive to risk-averse workers.

Non-participating women in our sample are more risk-averse than employed women, which is consistent with a participation cost risk. Our survey specifically asks workers for their reservation wages for a job in which they would be free to allocate their working hours across time. We are therefore able to focus specifically on worker-led flexibility.

2.2 Data and Descriptive Analysis

To analyse preferences for schedule autonomy, we use the SOEP 2010 pretest, a survey of just over 1300 respondents which includes a module of questions on labour market search, including preferences and reservation wages conditional on job characteristics as well as risk attitudes and a range of demographic and labour market characteristics. Jänsch and Siegel [2012] give details on the sampling method and structure. While the sampling strategy mirrors the main SOEP panel, the sample is distinct from the one used for the panel study and purely cross-sectional. Unlike the main panel sample which surveys all household members, the sample primarily centres on an individual and provides only limited household information. The sample is close to representative in terms of gender, employment, marital status and education, but there is a degree of over-representation of individuals aged 50 or over.

The key variables in our analysis are the questions on reservation wages:
“The decision whether someone accepts a job offer or not may depend on different surrounding conditions. I am going to give you different surrounding conditions a position may have. Please indicate for every case how high your net income would have to be for you to take the position that is offered. How how would your net income have to be to take the offered position if . . .

- . . . the new position had a fixed end of daily working time?

Table 16: Summary statistics for the whole labour force and estimation subsamples

	With non-missing WTP		Whole Labour Force	
	Mean	Std. Dev.	Mean	Std. Dev.
WTP for Schedule Autonomy	59.848	456.455		
as % of fixed-schedule RW	.01055	.1174		
Vocational	0.657	0.475	0.616	0.487
Academic	0.212	0.409	0.204	0.403
Age	43.19	11.95	43.83	13.42
Married (Binary)	0.526	0.500	0.525	0.500
Civil Servant	0.0299	0.171	0.0468	0.211
Not Full-time	0.348	0.477	0.446	0.497
From GDR	0.256	0.437	0.209	0.407
Gender	0.558	0.497	0.552	0.498
Children <16 in Household	0.331	0.471	0.306	0.461
Willingness to take Risks	5.015	2.397	4.936	2.459
Support from Workplace Network	0.214	0.410	0.190	0.393
Partner's support	0.421	0.494	0.370	0.483
Other family support	0.150	0.357	0.167	0.373
Fixed	0.409	0.492	0.418	0.494
Shifts	0.262	0.440	0.225	0.418
Informal	0.162	0.369	0.188	0.391
Flexitime	0.167	0.373	0.169	0.375
Observations	468		855	

WTP excludes one observation with a -900% willingness to pay. All remaining observations are between -66% and +100%. Willingness to Take Risks on a 0-10 scale. "Not full-time" includes part-time and marginal employment.

- . . . you did not had a formal working pattern but could adjust your schedule in a flexible way?”

The question immediately preceding this one asks about the reservation wage for a new job which “. . . was the same as your current job in its other aspects, that is, it had the same hours, same degree of flexibility and same commute?”. While it stresses three attributes, the question suggests that the respondent hold constant the whole bundle of other attributes. It is plausible that they will apply the same logic to the questions we use. We therefore interpret the two reservation wages as varying only the dimension of schedule autonomy and keeping all other aspects of the job identical to the current job.

Over a third of employed survey respondents (36%) do not answer at least one of the two reservation wage questions necessary to calculate a willingness to pay. There are no significant differences in propensity to answer both reservation wage questions by gender. Respondents with missing reservation wages are more educated, with the difference significant at the 10% level for both secondary and post-secondary education. They are also five years older on average, have larger incomes by €700 and are significantly less likely to have children under the age of 16 living in their household. A possible explanation is that, having accumulated a large amount of specialised human capital, these individuals are at a point in their career where they would be extremely unwilling to move to another job at all. Some of the missing reservation wages could thus represent “infinite” reservation wages. Civil servants are less likely to give reservation wages. This could be related to the low variance in the distribution of schedule autonomy within many civil-service careers, which may make these workers feel the scenario is not relevant to them. However, there are a number of competing explanations: the hypothetical scenario could be confusing to respondents, they may refuse to engage with it or be unwilling to disclose their reservation wages. The questions are significantly longer and more complex than many other questions in the survey, which may discourage respondents. It is therefore not possible to reliably infer that, for example, missing reservation wages must be larger than other reservation wages.

The gap between reservation wages for a job with and without schedule autonomy is a measure of workers’ willingness to pay. Controlling for a range of demographic characteristics, this measure significantly predicts preferences over fixed and autonomous scheduling of working hours elicited using more traditional ordinal preference scales. A high willingness to pay for schedule autonomy moreover strongly predicts workers working under a flexitime scheme at their current employer in a probit regression, showing

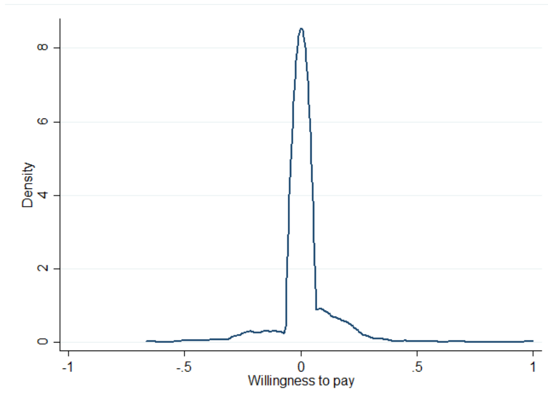


Figure 1: Distribution of willingness to pay measure

that reported reservation wages are predictive of real labour market choices.

Table 16 gives descriptive statistics. Conditional on strict positivity, mean willingness to pay for schedule autonomy is substantial at 16%. However, a large fraction of respondents reports the same reservation wage for jobs with and without schedule autonomy (see Figure 1). The implication that there is substantial heterogeneity in preferences is in line with other recent evidence [Mas and Pallais, 2017] from the experimental literature for workers in the US. On average, workers who prefer flexitime schedules are more risk-averse than other workers with the difference significant at the 5% level. However, this is mainly (in the case of male workers, entirely) due to the contrast with those workers who prefer shift work or complete independence. In contrast, those who prefer a fixed schedule are similarly risk-averse to those who prefer a flexitime schedule. Workers who prefer to have complete schedule autonomy, as well as those who are actually observed in such a schedule type, are the most risk-loving. This suggests that, as I outlined above, the flexible hours generated by such schemes are not predictable, a cost which may outweigh the advantage of autonomy for many workers.

Some of the management literature has argued that line manager support is a crucial determinant of the impact of "family-friendly" workplace practices on worker satisfaction [Beauregard and Henry, 2009]. In a similar vein, a recent inquiry of the Women and Equalities Select Committee was told, "One of the important things we need to understand is that flexible working itself is not necessarily an answer. It is a useful tool, but we are talking about workplace culture."[Jackson, 2017]. Contrary to this, we find that descriptively, workers who state that their co-workers or managers support their career give a lower willingness to pay for schedule autonomy, with the difference significant at the ten percent level overall and at the 0.1% level when the sub-samples are restricted to those workers with a positive willingness to pay. Workplace network support may act as a substitute for formal schedule autonomy provisions such as a flexitime scheme, if it allows workers to informally negotiate changes in their schedule or workload in response to home productivity fluctuations.

2.3 Estimation and Discussion

Table 17: Parameter estimates from a hedonic regression model (OLS)

	Gross Monthly Wage	
Age	9.576^{*}	(1.68)
Married (Binary)	205.5	(1.51)
Vocational	187.6	(1.01)
Academic	1160.0^{***}	(5.11)
Self-Employed	194.7	(0.77)
White-Collar	180.8	(1.12)
Civil Servant	836.1^{**}	(2.42)
Not Full-time	-1491.0^{***}	(-10.26)
East German	-741.3^{***}	(-5.03)
Female	-461.1^{***}	(-2.94)
Children	305.8	(1.42)
Female × Children	-181.5	(-0.67)
Shifts	64.51	(0.40)
Informal	355.3^{*}	(1.75)
Flexitime	756.3^{***}	(4.16)
Constant	1764.5^{***}	(5.90)
Observations	405	

t statistics in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. “Flexitime” refers to a formal scheme with some restrictions, e.g. core hours. “Informal” refers to complete worker autonomy over scheduling. “Not full-time” includes part-time and marginal employment. “East German” refers to the respondent’s place of residence before reunification. “Children” are children under the age of 16 living the respondent’s household.

In a hedonic regression of observed wages (table 17), workers with flexitime schemes earn a large and significant premium compared to workers with fixed schedules. Worker willingness to pay thus does not even come close to coinciding with the market wage premium, with the discrepancy potentially due to worker or firm heterogeneity or search frictions [Hwang et al., 1992, 1998].

As in Mas and Pallais [2017], most workers in our sample do not give a positive willingness to pay for schedule autonomy: A large fraction of workers reports a zero willingness to pay, and a small group reports a negative willingness to pay. There are a range of reasons why we might observe a zero willingness to pay, including rounding and the cost of cognitive effort. Workers may also legitimately have a zero or even negative willingness to pay. Whilst in a classical model, workers can never have a negative willingness to pay for what is effectively an additional degree of freedom, the cost of decision-making or self-commitment issues [Tan, 2019] may actually mean that some workers are willing to pay to have a schedule imposed externally. To deal with the spike of willingness to pay at zero as well as reduce the impact of outliers and functional form assumptions, our preferred specification is an ordered probit model (table 18). To estimate this model, we group responses into five categories of negative, zero, low (willingness to pay of up to 8% of the reservation wage), intermediate (up to 20%) and high (20% or more). Apart from the zero category (47% of all employed workers), these are similarly sized groups.

As we would expect from the theoretical discussion, women with children give a higher willingness to pay. Among childless workers, on the other hand, men state a higher willingness to pay than women. People with a vocational qualification give a higher willingness to pay than people without post-school qualifications or university graduates. Risk-loving workers state a higher willingness to pay than moderately risk-averse workers. There is no significant difference between moderately and highly risk-averse workers and when interaction terms are included, results suggest that the effect is driven by women and workers with children. One in five workers (but one in four women workers) is classed as highly risk averse.

Even though the questionnaire clearly asks about worker- rather than employer-led flexibility, task-based work with schedule autonomy may still generate risks associated with unpredictable hours, as discussed above. It is also possible that risk preferences are not stable across different domains, as the psychological literature has suggested, in which case the risk aversion question may inaccurately reflect the relevant preferences.

Table 18: Parameter estimates from an ordered probit specification of willingness to pay (main model) and extension with expectations (subsection 2.3.3)

	Willingness to pay for schedule autonomy			
Age	0.00731	(1.24)	0.00835	(1.38)
Married (Binary)	-0.179	(-1.24)	-0.117	(-0.79)
Vocational	0.392**	(2.09)	0.430**	(2.24)
Academic	0.159	(0.71)	0.196	(0.86)
Self-Employed	0.0658	(0.30)	-0.0616	(-0.27)
White-Collar	0.0420	(0.27)	-0.0210	(-0.13)
Civil Servant	-0.461	(-1.28)	-0.707*	(-1.85)
Not Full-time	0.124	(0.86)	0.0920	(0.63)
East German	-0.0596	(-0.44)	-0.0219	(-0.16)
Female	-0.275*	(-1.82)	-0.299*	(-1.92)
Children	-0.329	(-1.63)	-0.350*	(-1.70)
Female × Children	0.550**	(2.16)	0.610**	(2.34)
Risk Aversion: High	0.164	(0.95)	0.190	(1.08)
Low	0.267**	(2.07)	0.271**	(2.02)
Workplace Support	0.0809	(0.58)	-0.0121	(-0.08)
Partner's support	0.377***	(2.70)	0.403***	(2.83)
Other family support	0.211	(1.14)	0.358*	(1.87)
Expected: Shifts			0.0618	(0.40)
Informal			0.0998	(0.56)
Flexitime			0.390**	(2.33)
Constant 1	-0.619*	(-1.94)	-0.473	(-1.44)
Constant 2	1.713***	(5.22)	1.925***	(5.64)
Constant 3	1.989***	(6.01)	2.213***	(6.42)
Constant 4	2.505***	(7.37)	2.696***	(7.64)
Observations	443		431	

t-values in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Not full-time" includes part-time and marginal employment. "East German" refers to the respondent's place of residence before reunification. "Children" are children under the age of 16 living the respondent's household.

2.3.1 Willingness to Pay and Ordinal Preference Scales

A comparison with ordinal-scale preference measures, which are also included in the dataset, is instructive. We compare preferences on a 1-7 scale for three schedule types: a fixed schedule, worker-led independent scheduling and a structured flexitime scheme. Table 19 shows a probit regression where the outcome is a schedule type being weakly preferred to all others, with the sample restricted to the sub-sample giving reservation wages. The estimates indicate no difference between parents and childless workers and a strong preference of university graduates for worker-led schedules.

These differences between ordinal preferences and willingness to pay can be explained by different marginal utilities of the wage. The pattern is consistent with mothers having a lower marginal utility of the wage, for example due to specialisation. This could explain their higher willingness to pay compared to fathers and childless women, even with the same preferences for schedule autonomy. In the case of university graduates, their jobs may also vary more over other dimensions such as prestige or task content. Respondents would then struggle to give reservation wages for the parsimonious scenarios in the study and may default to giving equal reservation wages, even though they have a strong preference for schedule autonomy.

So although willingness to pay predicts sorting into schedule types and ordinal preference measures, it still is a distinct concept from ordinal preference measures. Not all workers who express a preference for a schedule type would be willing to trade off wages to achieve it as their marginal utilities from the wage also differ. Moreover, in a reference point framework, losses are more painful than gains are pleasant - this would make workers less willing to make a trade-off that takes them below their reference wage, even if it would leave them better off in terms of pure experienced utility. We will discuss this possibility further in subsection 2.3.3.

2.3.2 Complementarities

Workers who report that their partners support their career or educational progress are significantly more likely to report a higher willingness to pay for schedule autonomy. We interpret this finding as an indication that schedule autonomy is complementary to other support. This is consistent with home production requiring complementary time inputs from other members of the household. The effect is not driven purely by women or by workers with children.

Table 19: Parameter estimates from probit models of the preferred schedule type on an ordinal preference scale

	Fixed, constant		Flexitime		Informal	
Age	0.000648	(0.10)	-0.00926	(-1.46)	0.0111	(1.60)
Married (Binary)	0.0724	(0.46)	0.0765	(0.49)	0.00749	(0.04)
Vocational	-0.0241	(-0.12)	0.147	(0.73)	0.116	(0.53)
Academic	-0.730***	(-2.97)	0.592**	(2.44)	0.579**	(2.28)
Self-Employed	-0.621***	(-2.60)	-0.00682	(-0.03)	0.756***	(3.13)
White-Collar	-0.215	(-1.27)	0.0313	(0.19)	-0.0817	(-0.46)
Civil Servant	-0.101	(-0.24)	0.241	(0.57)	-0.402	(-0.88)
Not Full-time	-0.187	(-1.18)	0.201	(1.29)	0.461***	(2.80)
East German	-0.00430	(-0.03)	0.160	(1.10)	-0.0457	(-0.30)
Female	0.0817	(0.49)	-0.223	(-1.38)	-0.403**	(-2.27)
Children	0.195	(0.86)	0.0360	(0.16)	-0.121	(-0.51)
Female × Children	-0.376	(-1.33)	-0.0786	(-0.28)	0.408	(1.37)
Risk Aversion: High	-0.00144	(-0.01)	0.211	(1.13)	-0.142	(-0.70)
Risk Aversion: Low	-0.213	(-1.52)	-0.0272	(-0.20)	0.0763	(0.52)
Support: Workplace	-0.199	(-1.29)	0.133	(0.87)	0.0320	(0.20)
Partner's support	-0.0767	(-0.50)	-0.123	(-0.81)	0.102	(0.63)
Other family support	0.00107	(0.01)	0.139	(0.70)	0.0864	(0.41)
Constant	0.702**	(2.02)	0.124	(0.37)	-1.255***	(-3.34)
Observations	439		433		432	

t-values in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Outcome in column k: Schedule type k is weakly preferred to all others. Sample restricted to individuals with non-missing willingness to pay. "Flexitime" refers to a formal scheme with some restrictions e.g. core hours, "informal" refers to complete worker autonomy over scheduling. "Not full-time" includes part-time and marginal employment. "East German" refers to the respondent's place of residence before reunification. "Children" are children under the age of 16 living the respondent's household.

There are potentially important implications for public and workplace policy design. State and market resources may be insufficient to allow workers to fully exploit the benefits of schedule autonomy if they cannot rely on support from their partner. A report by Ruggeri and Bird [2014] for the European Union Programme for Employment and Social Solidarity based on 2010 labour force surveys shows that among 22 EU member states, Germany has the highest employment gap between young single mothers and young single childless women, who are also substantially more likely to be working full-time. The report specifically calls upon policy makers to consider the role of schedule autonomy in these discrepancies. Although we only observe a small number of single parents, our results on the importance of partners' support nevertheless suggest that schedule autonomy alone may not be enough to allow these workers to reconcile the demands of employment and household production.

2.3.3 Expectations and Reference Points

Reference points could influence both the underlying reservation wages and their reporting. Reference points can affect real job search decisions if workers' utility is asymmetrical over gains and losses both in terms of schedule autonomy and of wages. In addition to psychological loss aversion, this may also be due to spending commitments and arrangements for childcare and other home production that are costly to change. We find that workers who, based on the standard conditions in their sector, would expect to find a job with a flexitime arrangement in the event of a job change are much more likely to report a higher willingness to pay for schedule autonomy than those who expect other kinds of schedule arrangements (table 18).

Since Kőszegi and Rabin [2006] proposed using (rational) expectations as a measure of reference points, they have been used in many applications, including labour market applications such as income targeting. Intuitively, expectations are a better measure of reference points than the status quo: A worker who has been promised or otherwise expects a wage increase will consider a continuation of the status quo a disappointment because her reference wage is above her current wage. As in other applications, the status quo and expectations about future schedule types in our sample are correlated, but not identical: Close to half of workers currently on flexitime schemes and about a quarter of those on fixed schedules would expect a new job to have a different schedule arrangement to the one they are currently subject to. Interpreting our finding through this lens, workers who expect to find a job with a flexitime system would consider a

Table 20: Parameter estimates from an ordered probit model of the effects of earned wages and schedule types on loyalty

	Loyal to Employer	
Age	0.00192	(0.33)
Married (Binary)	-0.173	(-1.21)
Vocational	0.343*	(1.91)
Academic	0.788***	(3.07)
White-Collar	0.328**	(2.17)
Civil Servant	0.550	(1.51)
Not Full-time	0.288*	(1.69)
East German	0.0885	(0.56)
Gender	0.0322	(0.22)
Children <16 in Household	-0.0540	(-0.36)
Shifts	0.00435	(0.03)
Informal	-0.0518	(-0.22)
Flexitime	0.476**	(2.44)
None of the above	0.828	(1.31)
Wage	-0.0000457	(-0.71)
Constant 1	-0.997***	(-3.12)
Constant 2	-0.261	(-0.84)
Constant 3	0.133	(0.43)
Constant 4	0.711**	(2.28)
Observations	342	

t statistics in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Loyalty is self-reported on a 1-5 scale. Wages are gross wages. Excludes self-employed workers. "Not full-time" includes part-time and marginal employment. "East German" refers to the respondent's place of residence before reunification.

job with a fixed schedule as a loss, which would generate negative gain-loss utility in addition to the loss in experienced utility from the fixed schedule itself. This increases their willingness to pay.

Reference point effects could also affect rates of job-to-job mobility. To see this, consider the search model we discussed in subsection 2.1 with two-dimensional jobs consisting of a wage w and a job attribute x. Subject to stationarity conditions, a worker currently in job (w, x) should accept any offer $(w^\star, x^\star)$ that provides greater instantaneous utility $u(w^\star, x^\star)$ than the current job.

A special case of this model is one where the worker's utility incorporates reference points in the manner of Kőszegi and Rabin [2006]. They argue that the reference point consists of the subject's expectations in the recent past. In the case of voluntary job-to-job mobility, it is not unrealistic to suppose that the reference point is the current job, since the worker would have to actively give that job up to take a new one.

In that case, the instantaneous utility of quitting a job (w, x) in order to accept a new job offer $(w^\star, x^\star)$ would be

$$\begin{aligned} u(w^\star, x^\star|r) &= m(w^\star, x^\star) + n(w^\star, x^\star|r) \\ &= m(w^\star, x^\star) + \mu[m(w^\star, x^\star) - m_x(w, x)] \end{aligned}$$

where $m(\cdot)$ denotes experienced utility and $n(\cdot)$ denotes gain-loss utility with respect to the reference point r, which in this case corresponds to the current job (w, x).

Compare this model to a simpler one without reference point effects, where $u(w^\star, x^\star) = m(w^\star, x^\star)$. In the case of job offers which offer both a higher (lower) wage and a better (worse) attribute, both models make identical predictions about the acceptance (rejection) decision. But in cases where there is a trade-off to be made, the two models differ.[12] Because of loss aversion, formalised as assumption A2 in Kőszegi and Rabin [2006], workers in the reference point model will reject some offers that would be accepted in the standard model.[13]

[12] Note that this distinguishes this explanation from a general mobility cost, which would discourage *all* transitions with a low gain, whether this gain results from a slightly favourable trade-off or small improvements in both the wage and the attribute.

[13] Assumption A2 [Kőszegi and Rabin, 2006] states that for any $y > x > 0$, $\mu(y) + \mu(-y) < \mu(x) + \mu(-x)$. The sequence $(\mu(1/n))_{n\in\mathbb{N}}$ is therefore increasing. By continuity of μ and $\mu(0) = 0$, $\mu(y) + \mu(-y) < 0$.

This is because if the offer is rejected without reference points,

$$m(w^\star, x^\star) < m(w, x)$$
$$\Rightarrow \mu[m(w^\star, x^\star) - m(w, x)] < 0 \text{ by monotonicity of } \mu$$

i.e. gain-loss utility would make the offer even less attractive. So an offer that is rejected on the basis of experienced utility is *a fortiori* rejected once reference points are taken into account.

However, some offers that would be accepted in the standard model would be rejected in the reference point model, due to negative gain-loss utility outweighing gains in experienced utility. As a simple example, consider an additively separable utility function: $u(w^\star, x^\star|r) = m_w(w^\star) + \mu[m_w(w^\star) - m_w(w)] + m_x(x^\star) + \mu[m_x(x^\star) - m_x(x)]$ and a job offer $(w^\star, x^\star)$ which in terms of experienced utility leaves the worker just slightly better off: $m_w(w^\star) + m_x(x^\star) = m_w(w) + m_x(x) + \epsilon$ for some $\epsilon > 0$. Let the gain be asymmetrical, i.e. a wage loss is more than compensated for by a better attribute: $m_w(w^\star) - m_w(w) < 0 < m_x(x^\star) - m_x(x)$. The worker without a reference point would accept this offer. But in a reference point model, we can choose an ϵ sufficiently small such that $\mu[m_w(w^\star) - m_w(w)] + \mu[m_x(x^\star) - m_x(x)] < -\epsilon < 0$.

The set of acceptable job offers with reference points is therefore a true subset of the set of acceptable offers in the corresponding simple additive-utility model without reference points.

It is interesting to note that workers who report having an informal schedule arrangement do not have a similarly high willingness to pay as those on flexitime schemes. In part, this may be due to the heterogeneous nature of this group. But informal schedule arrangements may also be associated with more uncertainty over working hours and not deliver the same benefits in terms of additional home production as formal flexitime schemes. In the UK, Bryan and Sevilla [2017] find that whereas flexitime increases couples' ability to coordinate their schedules, other types of flexible working does not.

With regard to effects on reporting rather than actual willingness to pay, the first reservation wage elicited in the survey is one for the same schedule as in the current job. This wage could act as a reference point in the interview and workers, especially those who currently work fixed hours, may be averse to reducing the reservation wage they state in subsequent questions.

Table 21: Parameter estimates from an ordered probit model of the effects of earned wages and schedule types on perceived fairness of wage

	Fairness of Current Wage	
Age	-0.0224***	(-3.99)
Married (Binary)	0.291**	(2.19)
Vocational	0.240	(1.35)
Academic	0.282	(1.21)
White-Collar	0.0733	(0.50)
Civil Servant	0.121	(0.39)
Not Full-time	0.296*	(1.88)
East German	-0.347**	(-2.36)
Gender	0.0327	(0.24)
Children <16 in Household	-0.311**	(-2.28)
Schedule Type: Shifts	-0.281*	(-1.95)
Informal	0.0749	(0.33)
Flexitime	0.285*	(1.65)
Other	-0.865*	(-1.73)
Wage	0.000152***	(2.59)
Constant 1	-1.912***	(-6.01)
Constant 2	-1.618***	(-5.17)
Constant 3	-1.023***	(-3.33)
Constant 4	-0.573*	(-1.88)
Constant 5	-0.273	(-0.90)
Constant 6	1.507***	(4.76)
Constant 7	1.753***	(5.41)
Constant 8	2.649***	(5.84)
Observations	345	

t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Ordered Probit. Fairness rating and earned wages both refer to gross wages. Excludes self-employed workers. “Not full-time” includes part-time and marginal employment. “East German” refers to the respondent’s place of residence before reunification.

2.3.4 Fairness and Effort

Akerlof and Yellen [1990] introduced the fair wage-effort hypothesis, which postulates that when wages fall short of a reference point in employment situations with imperfect monitoring, workers provide less effort. The hypothesis originates with the sociological gift exchange paradigm. An employer's decision to pay wages below the reference wage is interpreted as a hostile act capable of triggering retaliation.

Table 22: Parameter estimates from a probit model of the effects of earned wages and schedule types on perceived fairness of the wage in the main SOEP

	Pooled	Pooled	Random Effects
Current Wage Fair			
Age	-0.00249*	-0.00284*	-0.00267
	(-2.69)	(-3.06)	(-1.45)
Married	0.0547*	0.0550*	0.0555
	(2.81)	(2.83)	(1.48)
Vocational	-0.0794*	-0.0802*	-0.145
	(-2.82)	(-2.84)	(-2.57)
Academic	-0.259***	-0.264***	-0.419***
	(-7.47)	(-7.59)	(-6.04)
White-Collar	0.0295	0.0304	0.0706
	(1.44)	(1.48)	(1.89)
Civil Servant	0.260***	0.256***	0.455***
	(7.35)	(7.20)	(6.37)
Part-Time	0.538***	0.537***	0.712***
	(21.13)	(20.69)	(15.73)
East German	-0.308***	-0.304***	-0.515***
	(-16.39)	(-16.11)	(-12.93)
Female	0.0770**	0.0827***	0.168***
	(3.82)	(4.09)	(4.16)
Children	-0.0134	-0.0147	-0.0247
	(-0.71)	(-0.78)	(-0.63)
Wage	0.000241***	0.000254***	0.000333***
	(16.58)	(14.22)	(10.63)
Wage Squared	-4.61e-09*	-8.03e-10	1.78e-09
	(-3.19)	(-0.42)	(0.54)
Shifts	-0.131***	-0.101	-0.184*
	(-6.29)	(-2.48)	(-2.92)
Informal	0.165***	0.401***	0.533***
	(6.24)	(8.91)	(7.28)
Flexitime	0.150***	0.159*	0.216*
	(6.52)	(3.09)	(2.58)

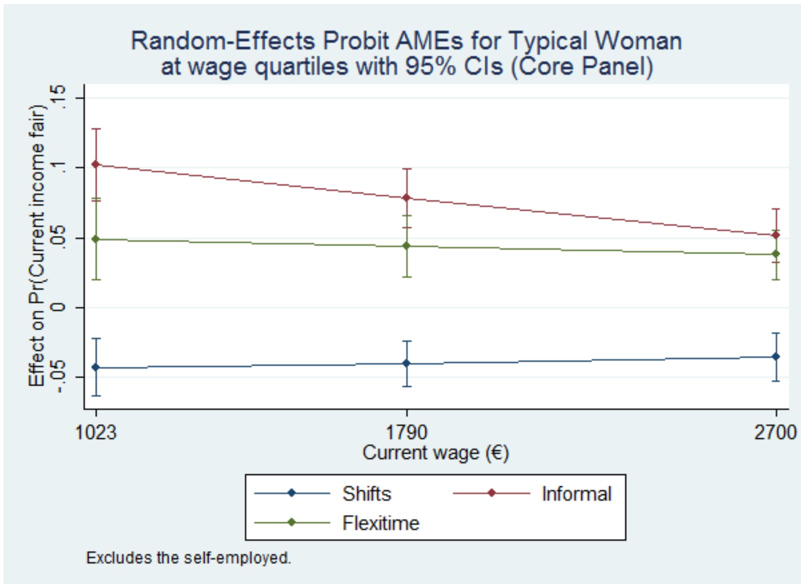
Year=2007	-0.252***	-0.252***	-0.395***
	(-12.05)	(-12.03)	(-14.44)
Year=2009	-0.117***	-0.117***	-0.171***
	(-5.25)	(-5.21)	(-5.76)
Year=2011	-0.0995***	-0.1000***	-0.146***
	(-4.14)	(-4.15)	(-4.47)
Shifts × Wage		-0.0000152	0.0000116
		(-0.89)	(0.44)
Informal × Wage		-0.0000925***	-0.000112***
		(-6.20)	(-4.63)
Flexitime × Wage		-0.0000145	-0.0000179
		(-0.86)	(-0.65)
Constant	-0.0493	-0.0862	-0.0223
	(-0.94)	(-1.55)	(-0.21)
Log Variance			
Constant			0.437***
			(10.61)
Observations	27349	27349	27349

t statistics in parentheses, * $p < 0.01$, ** $p < 0.001$, *** $p < 0.0001$. Excludes the self-employed. "Wage" is the gross monthly wage. "East German" refers to the respondent's place of residence before reunification in 1989. "Children" are children under the age of 16 living in the respondent's household.

This implies that, in addition to the reservation wage which determines job offer acceptance, there is a second important wage threshold, that of the "fair" reference wage. Parts of the literature [Falk et al., 2006, for example] have used the two concepts interchangeably and postulated that workers would reject offers they consider to be unfair. Certainly, the set of unacceptable wages would be a subset of the set of unfairly low wages. However, the Akerlof-Yellen model specifically focuses on workers who are employed, so their wage must be above their reservation wage, but potentially below their reference wage. This is consistent with a descriptive analysis of our data: When asked to place their current gross wage on an eleven-point scale between "unfairly low" and "unfairly high", only just over half (52%) of respondents state that their wage is equal to or more than the fair wage, which leaves a very substantial minority who perceive their wages to fall in the interval between the reservation wage and the fair wage.

In the original model, only the wage enters the effort provision decision. However, it seems likely that workers would also consider other job attributes to gauge whether

Figure 2: Probit marginal effects from specification (3) in table 22 for a woman at the 25th, 50th and 75th wage percentile, of median age (45), with all discrete covariates set at their mode.



the job as a bundle provided by the employer should be interpreted as a "friendly" or a "hostile" action, if these attributes are costly for the employer to provide. This is analogous to the extension of the reservation wage to a reservation utility and can equally generate a willingness to pay, namely the marginal rate of substitution between the wage and the job attribute at the reference utility. Depending on the shape of the utility function, this substitution rate could be the same or different from the substitution rate at the reservation utility.

Testable predictions of the fair job offer-effort hypothesis Our data does not give a direct measure of this trade-off. However, we can test a number of predictions of this extended fair wage-effort hypothesis, which we will hereafter refer to as the fair job offer-effort hypothesis. Table 20 shows that workers on flexitime schedules report a greater sense of loyalty towards their company, which is consistent with the theory of gift exchange and would be expected to reduce shirking, one of the predictions of Akerlof and Yellen [1990].

Moreover, if there is a trade-off between the wage and job attributes in the reference utility, a given wage is more likely to be considered fair if it is accompanied by a

Table 23: Parameter estimates from a probit model of the effects of earned wages and schedule types on self-reported effort

	Effort		Effort	
Wage	0.0000408	(0.95)	0.00000503	(0.08)
Schedule: Shifts	0.322**	(2.26)	0.276*	(1.88)
Informal	0.193	(0.87)	0.175	(0.76)
Flexitime	0.326*	(1.88)	0.287	(1.59)
Other	0.549	(1.00)	0.556	(0.99)
Age			-0.00553	(-1.01)
Married (Binary)			0.0135	(0.10)
Vocational			0.497***	(3.00)
Academic			0.633***	(2.84)
Civil Servant			-0.196	(-0.65)
Not Full-time			-0.0881	(-0.56)
East German			0.00562	(0.04)
Gender			0.163	(1.23)
Children			-0.0424	(-0.30)
Constant 1	-1.876***	(-10.66)	-1.785***	(-5.97)
Constant 2	-1.276***	(-9.57)	-1.164***	(-4.23)
Constant 3	-0.730***	(-6.02)	-0.610**	(-2.26)
Constant 4	0.306***	(2.60)	0.455*	(1.69)
Observations	374		372	

t statistics in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Ordered Probit. Excludes the self-employed. "Wage" is the gross monthly wage. "Not full-time" includes part-time and marginal employment. "East German" refers to the respondent's place of residence before reunification in 1989. "Children" are children under the age of 16 living in the respondent's household.

favourable job attribute. In our case, this would mean that workers with schedule autonomy would achieve their reference utility with a lower wage than those with a fixed schedule. Table 21 shows an ordered probit model of the fairness rating (on an eleven-point scale) of workers' own gross wage. Workers on flexitime schedules are less likely to consider their gross wage unfairly low, given the actual wage they are earning.

This finding can be substantiated by the main SOEP panel, which contains a binary indicator of whether respondents consider their current wage to be fair. This loses some information, but the greater sample size enables much more precise coefficient estimates. 63% of respondents assess their own wage to be fair, whereas in the pretest, only 53% choose a non-negative rating on the 11-point scale, representing an exactly fair or more than fair wage. This could represent differences between the two samples, but could also mean that some of those rating their wage as slightly too low to be fair would still answer yes to the binary question.

Table 24: Share of flexitime work by worker and job characteristics in the Pretest and main panel

	Pretest	Main		Pretest	Main
Age Group			**Sector**		
20-30	0.14	0.17	Industrial	0.1	0.068
30-40	0.22	0.21	White-Collar	0.23	0.26
40-50	0.21	0.21	Civil Servant	0.28	0.33
50-60	0.18	0.2	**Full-time**	0.21	0.21
60 and over	0.27	0.16	Others	0.18	0.13
Marital Status			**East German**	0.22	0.18
Not married	0.21	0.18	Others	0.19	0.2
Married	0.18	0.19	**Gender**		
Post-School Education			Male	0.23	0.2
None	0.12	0.089	Female	0.17	0.18
Vocational	0.17	0.17	**Children**	0.22	0.21
Academic	0.34	0.28	No children	0.19	0.18
Observations	593	92,639			

Excludes the self-employed and respondents who are students or pensioners as their primary status. "East German" refers to the respondent's place of residence before reunification in 1989. "Other than full-time" includes part-time and marginal employment "Children" are children under the age of 16 living in the respondent's household.

Furthermore, although the necessary variables only overlap for four out of the 31 waves of SOEP, the panel structure allows us to account for unobserved heterogeneity in this model. Table 22 firstly shows results for an near-equivalent model to the one estimated on the Pretest sample. Making use of the greater sample size and panel structure, the effect of the wage is subsequently allowed to vary by schedule type and a panel model with random effects. Image 2 shows that flexitime workers are substantially more likely to consider their wage fair, as are workers who have complete schedule autonomy without a formal arrangement (note that self-employed workers are excluded from this model). The effect for flexitime workers is remarkably stable across the wage distribution, whereas for workers without a formal schedule arrangement, the effect is largest for low wages. Wages among workers with informal schedule arrangements are higher on average and more dispersed than for other workers, such that the 75th percentile of all wages (the "high" wage in the graph) remains below the mean for workers on informal schedules.

The hypothesis would also predict that given their wage, workers on flexitime schedules will exert more effort. Our dataset provides some categorical information on effort. Its distribution is highly skewed: 87 % of respondents say they rather agree or completely agree with the statement that they go above and beyond the required level of effort. The descriptive evidence supports the fair job offer-effort hypothesis: Given their wage, flexitime workers are more likely to report exerting more effort than their job description requires in order to support their firm. The effect is significant (p=0.06) in an ordered probit model controlling for the wage and schedule type, but loses significance (p=0.11) once we additionally control for workers' demographic characteristics such as age, education and marital status (table 23).

The prevalence of flexitime work varies widely across these dimensions (table 24), likely reflecting a combination of preference-based sorting and different offer distributions. Workers on shifts also exert significantly more effort, which cannot easily be explained by the fair job offer-effort hypothesis. We do not have information on respondents' occupation, but it is likely that shift workers include a high proportion of pro-social occupations such as medical professionals, police officers, fire fighters, etc. Workers in these occupations are likely to have a high degree of intrinsic motivation leading to high effort levels, independently of wages and job characteristics.

Conclusion

We have used an innovative survey data set including reservation wages conditional on schedule type to study differences in willingness to pay for schedule autonomy. We have shown that gender, parenthood, education and career support by one's partner matter for willingness to pay and that willingness to pay measures a related but distinct concept from pure preference. Theory suggests that if certain time inputs normally allocated towards market work are very productive in home production or if productivity in home production fluctuates, workers will benefit most from schedule autonomy. This is a likely mechanism behind the result that women with children exhibit a high willingness to pay.

We have also found some evidence of reference points affecting worker's evaluations of wage-schedule type dyads at the offer acceptance and effort margins. Additional data collection efforts, ideally combining external measures of effort with the information on wages and schedule autonomy we used, could throw additional light on this relationship.

Similarly, given the results on differences between mothers and childless women, panel data that would allow an analysis of changes in willingness to pay around childbirth would be promising.

3 Have Minimum Wages Affected Schedule Flexibility in the UK?

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In this paper, I examine the effect of eleven increases in the prevailing minimum wage rate for adult workers[14] in the United Kingdom on schedule amenities and disamenities. It is intuitive that a binding minimum wage might affect the distribution of on non-wage amenities, including schedule types valued by workers, among employed workers if employers attempt to keep the value of the overall job bundle constant.

However, previous theoretical and applied work have shown that compensating differentials for non-wage amenities are often absent or wrong-signed in markets with search frictions [Hwang et al., 1998, Bonhomme and Jolivet, 2009]. The introduction or increase of a minimum wage causes a wage change that is exogenous to worker productivity, and more exogenous to firm productivity than most other wage variation. Therefore, this application has the potential to generate interesting insights on the wage-amenity relationship in markets with low worker bargaining power.

Moreover, there are several reasons why this margin is an important one for policy makers to consider: Firstly, if employers respond to higher minimum wages by reducing job quality along non-wage dimensions, this unintended consequence could hurt the very workers that the minimum wage is intended to help.

Secondly, if preferences over wages and non-wage amenities are heterogeneous, an increase in the wage and compensating deterioration in an amenity would have a particularly negative effect on workers with a high willingness to pay for these amenities. This is especially important in the case of flexible schedules, the amenity I focus on in this paper. A growing literature shows that worker valuation for schedule flexibility is heterogeneous and the marginal worker has a substantial willingness to pay [Mas and Pallais, 2017]. If minimum wage legislation reduces the provision of flexible schedule types that are costly to employers, this would have a disproportionate impact on workers with a high valuation for those schedules, including women with caring responsibilities.

Thirdly, the scope for "consolidating" additional payments and job amenities into

[14] ten upratings of the National Minimum Wage and the introduction of the National Living Wage

base wages is likely to decrease with further increases in the minimum wage's bite [Neumark, 2018], since most job attributes are constrained from below by law, nature or both. If this adjustment margin is very important when a minimum wage is first introduced or when it is increased from a low level, then this is one reason to expect stronger employment impacts for subsequent increases at higher levels, when these options have been exhausted. This is becoming increasingly relevant as the policy debate in the US as well as in Europe is seriously considering much more substantial minimum wage increases than in the recent past. In particular, both major parties' platforms in the 2019 UK General Election included a commitment to increasing the National Living Wage to beyond ten pounds an hour. Given the evidence that compensating differentials often do not arise in markets with search frictions, it is not certain a priori whether we should expect an effect of an increase in wage costs on job attributes.

What is more, some contributors to policy debates around the minimum wage argue that the effect could go in the opposite direction. This argument is based on multiple equilibria, with minimum wages allowing policymakers to select a "good" equilibrium. In this equilibrium, a more productive set of firms offer jobs that are both higher-paid *and* have better attributes. In this case, the effect on job attributes would *strengthen* the case for the minimum wage. In summary, the effect of minimum wage policies on job attributes is an essential and under-researched part of a comprehensive evaluation of its (re-)distributive effects, as well as providing an interesting source of wage variation to study the wage-amenity trade-off.

In this paper, I use a well-established methodology to study effects of minimum wage increases on an understudied, but potentially very important margin. I find that in regions and industries most affected by increases in minimum wages in the UK, schedule disamenities have become more prevalent. In particular, increases in the impact of minimum wages are associated with the expansion of zero-hours contracts. There is some evidence of a corresponding effect which decreases schedule amenities, but the picture is less clear. The paper is structured as follows: The next section summarises the existing literature, both theoretical and empirical, on the effects of a minimum wage on job attributes. Section 3.2 presents potential mechanisms, which I expand upon in an appendix (Section 3.3.3). Section 3.3 describes the sample, sets out the empirical approach using a fixed-effects regression and a dynamic model and presents contextualising results on wages and employment as well as the main results on schedule amenities and disamenities. Section 3.3.3 presents robustness checks and extensions to the main specifications.

3.1 Literature

Both traditionally and in the current literature, the focus of the minimum wage literature has been on employment and wages. Nevertheless, from the beginning of this literature, there has also been an interest in other adjustment margins. These include prices, capital, different types of labour inputs, quality [Giupponi and Machin, 2018] and, most importantly for this paper, non-wage job attributes. Many early contributions infer an effect on the utility provided by the whole job bundle from indirect evidence on outcomes such as labour force participation, prices, application and quit rates. Wessels [1980] argues that full compensation fell with minimum wage increases in the US in the 1960s, based on an implicit assumption of a perfect market for job attributes. In addition to indirect evidence from quit rates, prices and participation, he also quotes a firm survey from New York where a large share of firms reported reducing worker-friendly job attributes in response to an increase in a sector- and region-specific minimum wage hike.

In contrast, Holzer et al. [1991] find that there is queuing for minimum-wage jobs, which they interpret to mean that wage increases induced by the minimum wage are not fully offset by a deterioration in job attributes. However, Sicilian and Grossberg [1993] argue that this application behaviour is based on a misperception of job attributes on the part of workers, who overestimate the overall utility from a job at the increased minimum wage and only learn about the full vector of job characteristics when they are already working.

Acemoglu [2001] conceptualises job quality somewhat differently: He presents a model of a segmented labour market, where good jobs pay negotiated wages that include rent sharing, but bad jobs pay low wages and are bound by the minimum wage. A higher minimum wage increases the number of good jobs, as well as the value of unemployment. A 1996 working paper version of the same paper included an empirical application where he defined "good" jobs as jobs in occupations whose dummy has a positive coefficient in a Mincer-type wage regression and analysed their share across states and time. He finds that higher minimum wages increase the number of good jobs.

Amongst the wide range of specific attributes that are of interest, many researchers have chosen to focus on minimum wage effects on employer-sponsored training. Here, too, early work relied heavily on indirect evidence, especially experience-wage profiles, to infer effects on training [Lazear and Miller, 1981, Hashimoto, 1982]. However, there are a number of competing explanations for their findings of flatter wage profiles, including the possibility that minimum wages lead to a "frontloading" of the earnings path [Brown, 1999]. In addition to not being able to distinguish between these competing explanations of reduced training on the one hand and a change in the earnings path which keeps lifetime compensation constant, the methods used in this earlier part of the literature are not able to account for search. Job attributes, firm and worker heterogeneity cannot usually be disentangled.

The more recent empirical literature has therefore focused on direct measurement of specific job attributes. The seminal paper by Card and Krueger [1994] includes an analysis of free or discounted meals for fast-food industry workers - a very specific non-wage amenity - and find no effect.

Health insurance and employer-provided pensions have also been studied in the US context. Royalty [2000] studies the effect of state-level variation of minimum wages on workers' eligibility for health insurance and pensions between 1988 and 1993. She finds that large increases, or increases at already high levels, lead to a decline in workers' eligibility for these benefits. Simon and Kaestner [2004] on the other hand, find no effect on health insurance and pensions using different measures of coverage and variation in both federal and state minimum wages over the period between 1979 and 2000. In a recent contribution, Clemens et al. [2018] find that state-level minimum wage in the period from 2011 to 2016 decreased the likelihood of individuals reporting employer-sponsored health insurance plans. They use detailed occupational information to determine which workers are most likely to be affected by minimum wage increases.

The literature on training has found mixed results for different institutional contexts and time periods. Along with the rest of the minimum wage literature, much of the work on the effects on non-wage job attributes has embraced difference-in-differences strategies in the aftermath of Card and Krueger's work. Neumark and Wascher [2001] find a decrease in formal training of young workers in the US in 1991. In contrast, Fairris and Pedace [2004], using firm-level variation in minimum wage bite in the US, find no effect on training intensity in the US in 1997 and weak evidence of a reduction in incidence.

In Europe, Schumann [2017] analyses the effect of the introduction of a sectoral minimum wage in construction in Germany in 1997 on the provision of apprenticeships. There is no possibility of a direct effect in this setting, only a spillover effect, or an anticipation of reduced labour demand in the future. This is because the minimum wage under study applied to workers with a completed apprenticeship, rather than incumbent or newly hired apprentices. The sectoral nature of the minimum wage lends itself especially well to a difference-in-differences strategy. He finds a negative effect on firms' decision to train any apprentices, as well as on the number of apprentices trained. Heterogeneity is important: The effects are much stronger in the East, where the minimum wage bite was higher.

Arulampalam et al. [2004] find no evidence for a decrease in training caused by the UK National Minimum Wage, and some evidence of an increase. Their difference-in-differences strategy is based on workers' position in the wage distribution or workers' own reports of whether their wages were raised in response to the introduction of the National Minimum Wage. This definition means that the treatment effect does *not* include spillovers to workers whose wages may be indirectly affected by the minimum wage. This, in addition to differences in institutions, could contribute to the difference between their findings and those of Schumann [2017].

In the labour search literature, a number of economic models have set out effects of minimum wages in segmented labour markets with "good" and "bad" jobs of varying definitions, including Drazen [1986], Jones [1987], Acemoglu [2001] and Cahuc et al. [2001]. Acemoglu's model predicts an increase in good jobs in response to the introduction of a minimum wage, whereas the other models make mixed or in some cases no predictions. The framework set out by Clemens et al. [2018], which does not rely on segmentation, also predict a reduction in amenity provision, at least for certain ranges.

Looking more specifically at the literature analysing the history of the UK National Minimum Wage and National Living Wage, Papps and Gregg [2014] use wage gap specifications in ASHE and the LFS to examine the effect of increases in the NMW between 1997 and 2011 on labour market flows, as well as a wide range of job attributes, including hours and weeks worked, employer-provided pensions and a range of schedule types, as well as work from home. They find no effect on any schedule type. The evidence on work from home they find is mixed, which, as another type of flexibility of value to workers, could be considered a related job attribute to worker-friendly flexible schedules. This result is for working mainly from home and conditional on not changing jobs.

There are a number of reasons why this paper complements their evidence: Firstly, theirs is an earlier paper, during whose period of observation zero-hours contracts were very rare, and the adjustment mechanisms to subsequent minimum wage increases may have changed. Secondly, they use a wage gap specification, which defines treatment intensity using the gap between the current wage and the incoming minimum wage. A group of workers who already earn slightly more than the incoming minimum wage is used as a control group. This specification captures direct effects on those whose wages have to be raised to comply with the incoming minimum wage. However, many schedule types are not easily implemented for individual workers, but instead enable or even require some degree of restructing of the firm's processes. It is therefore plausible that workers in firms or industries that are strongly affected by a minimum wage increase could experience spillover effects on the schedules they work, even if their own wages are unaffected. Allowing for spillover effects is therefore an interesting extension to the earlier work.

A recent paper by Datta et al. [2019] shows that the introduction of the National Living Wage in 2016, the largest minimum wage increase in my observation period, was associated with an increased use of zero-hours contracts, an employer-friendly type of schedule flexibility, in the care home sector using the National Minimum Dataset for Social Care. They also use the Labour Force Survey to show that individual workers in low-wage industries were more likely to be on zero-hours contracts after the introduction of the National Living Wage. The two papers were developed independently from one another, and I complement their evidence in several ways: Firstly, I study a broader range of schedule types, including those beneficial to workers, and discuss the potential dual nature of zero-hours contracts as a schedule and a job security disamenity. Secondly, I use variation from a longer time series of minimum wage increases, and combine variation over time with variation across regions and industries in Great Britain. Finally, I allow for dynamic effects in an Arellano-Bond specification and account for differences in productivity growth using region-industry level gross value added.

Previous examples in the UK literature for a regional/industry-level approach similar to the one I use in this paper are Dickens and Manning [2004], Stewart [2002] and Dolton et al. [2012], all of whom focus on wages and employment as outcomes. They find that minimum wages significantly increased wages at the bottom of the distribution, but spillovers on non-treated workers were small. They did not find any disemployment effects. In an appendix to his recent report, Dube [2019] used a similar approach to analyse the effect of the introduction of the National Living Wage, the single biggest reform in my period of observation, finding no impact on either headcount or full-time equivalent employment.

This chapter complements the analysis in the previous two chapters in several ways: Firstly, I focus on the low-wage labour market. This is a segment where women are overrepresented and where workers have very limited bargaining power. I show evidence that minimum wage increases are associated with increased prevalence of schedule disamenities, particularly in industries with a high share of women workers. Combined with the evidence from the two previous chapters on differences in willingness to pay for non-wage amenities, this suggests that minimum wage policies may have unequal distributional consequences.

3.2 Mechanisms

Economic theory predicts a number of different channels for a higher wage floor to affect the provision of job attributes such as schedule amenities. The direction of effects is not always clear ex ante. In this section, I will illustrate two of those channels that are likely to be relevant to the case of schedule amenities.

Pay consolidation To begin with, when a minimum wage is introduced or increased, employers may decrease the provision of costly attributes to counteract the associated increase in cost. This "consolidation" of amenities into basic pay also reduces the impact of the minimum wage increase on workers' utility. Manning [2003, p. 326] makes this argument, stating that "[i]f there are non-pecuniary aspects to job (call them effort), then one would expect employers to raise effort in response to a minimum wage [...] (although it is fair to say that no evidence for these off-setting effects has ever been produced)." In this paper, I think of job attributes primarily as positively valued amenities, or *reductions* in effort. At its most basic, this argument predicts a reduction in worker-friendly schedules and additional payments specifically among workers whose wages were increased to comply with the National Living Wage, and among new hires at the National Living Wage. The Commission [2017, p. 61] reports that some stakeholders told them about cuts to additional payments or switches to (less worker-friendly) zero-hours contracts in response to an increase in labour costs induced by the National Living Wage.

However, if employers have some monopsony power in the sense that a cut in total compensation does not lead to employees leaving immediately, they may also decrease the provision of costly attributes for workers higher up the wage distribution. There is some empirical support for the idea that off-setting actions by employers need not be limited to those workers directly affected by a new statutory wage floor [e.g. Clemens et al., 2018, for health insurance]. In another example, Bellmann et al. [2017] analyse the introduction of a minimum wage in Germany in an establishment-level difference-in-differences framework and find that highly impacted employers decreased training for medium- and high-skilled employees, rather than the low-skilled employees whose wages had to be increased to comply.

A pay consolidation mechanism is intuitively plausible, and present across a range of theoretical models. In a model without search, a firm seeking to minimise costs will choose an optimal combination of wages and job attributes and a compensating wage differential will arise. If the optimal bundle becomes infeasible after the introduction of the minimum wage, we would expect to see an adjustment on the margin of (non-legally mandated) attributes. A similar mechanism is present in a richer model with search and differences in the cost of providing attributes. I sketch out the mechanism in such a model, building on the search models of Gronberg and Reed [1994], Hwang et al. [1998] and van den Berg [2003], in an appendix (Section 3.3.3).

The same intuition is formalised in Clemens et al. [2018]. They focus on a situation where firms are homogeneous in their cost of providing non-wage amenities. This is plausible for amenities with direct monetary costs like health insurance, which they analyse, but less so for amenities whose cost is primarily in lost production or increased need for coordination, like worker-led flexible schedules. In the latter situation, Hwang et al.'s model [1998] with differences in production technology for job amenities is more realistic.

Clemens et al. [2018] also assume that the reservation utility is exogenously fixed. Van den Berg [2003] emphasises that in a search model, the equilibrium will depend on workers adjusting their reservation wage (or utility) in response to the distribution of offers of those firms remaining in the market. This has implications for the wage (or utility) distribution in a market with a minimum wage. However, the qualitative prediction (for a certain parameter range) that some firms will exit while others will adjust their offer, is similar.

Differentials The role of a minimum wage as a reference point for a fair wage [Falk et al., 2006, Fedorets et al., 2018] may have negative consequences on morale, effort and/or turnover of employees previously paid at or close to the new minimum wage rate. Employers could improve attributes of these jobs to preserve utility differentials.

Previous studies have found that the National Minimum Wage, introduced in 1999, has not led to wage spillovers high up the distribution [e.g. Dickens and Manning, 2004], but the attribute distribution could be an alternative channel for positive spillovers. This channel predicts positive effects on non-wage amenities for workers not directly affected by the new wage floor, i.e. workers whose pre-policy wage was close to, but not below the new wage floor.

3.3 Empirical Analysis

I use a region-industry level approach to analyse the effect of minimum wages on job attributes. I exploit the fact that a uniform minimum wage floor such as the National Living Wage will affect regions and industries to varying degrees due to pre-existing differences in the wage distribution. I expect regions and industries where the minimum wage reaches far into the wage distribution to experience larger effects on job attributes than regions and industries where the minimum wage is low relative to the median wage.

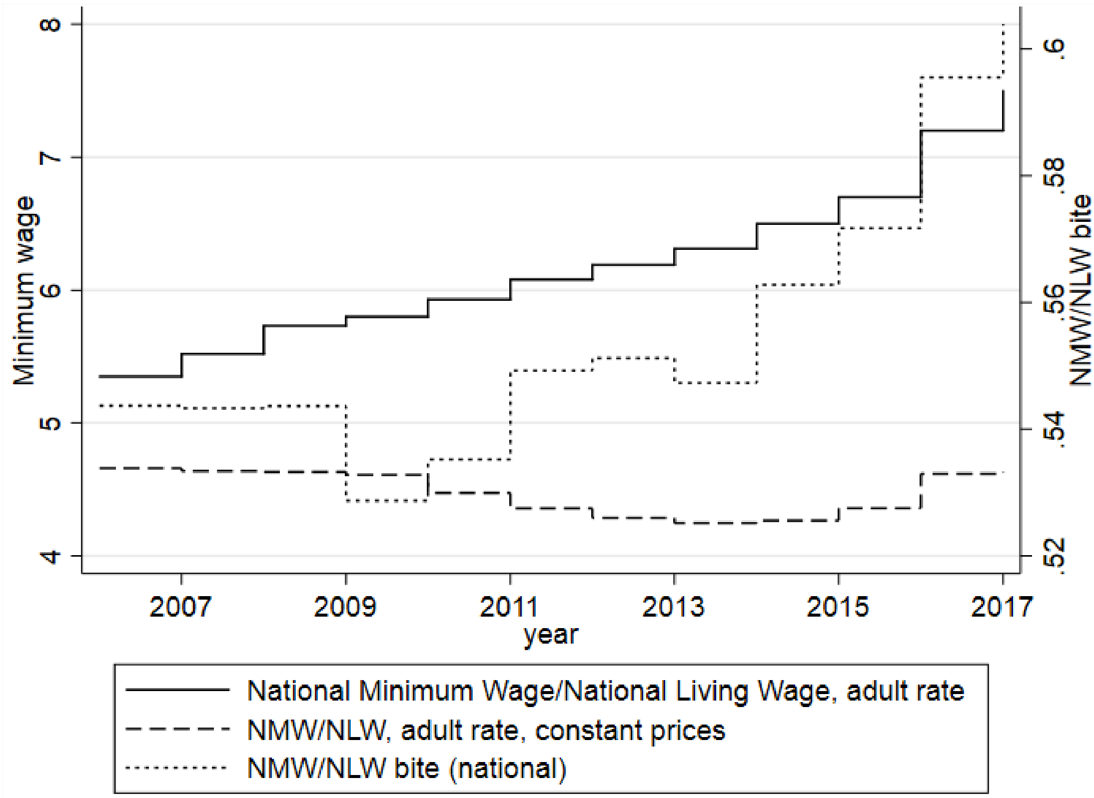
Terminology and institutional context In this paper, I will refer to "the minimum wage" to mean the adult rate of the National Minimum Wage in the period between 2006 and 2015, and the National Living Wage in 2016 and 2017[15]. Treatment intensity varies across region-industry cells and across time, since the National Minimum Wage and the National Living Wage are uprated every year.[16]

Upratings can be of different magnitudes and while the decision is ultimately the government's, it usually follows the recommendation of the Low Pay Commission. The commission is an arms-length body with members representing workers', employers' and academic perspectives. Its terms of reference task the commission with using the best available evidence to recommend a rate that "will help as many low-paid workers as possible without any significant adverse impact on employment or the economy"[Commission, 1998]. The remit of the commission also includes a policy goal to increase the National Living Wage to 60% of median earnings by 2020, with a scientific review currently in progress to determine a goal for the future. Figure 3 shows the

[15]The National Living Wage replaced the National Minimum Wage for workers aged 25 and over in 2016. It is not to be confused with the Living Wage (sometimes called the "real Living Wage"), which is a voluntary certification scheme for employers overseen by the Living Wage Foundation, with rates explicitly based on cost of living.

[16]Exceptionally, in 2016, there were two upratings, however the second one only changed the rates for young workers and apprentices, leaving the adult rate unchanged.

Figure 3: Minimum wages over time. Price adjustment using the Retail Price Index (RPI) of all items except mortgage interest.



evolution of the National Minimum Wage and the National Living Wage over time.

3.3.1 Specification

In this section, I will first set out my empirical approach and define central concepts, before giving detail on the data sources used.

Treatment Intensity Some previous papers [e.g Stewart, 2002] dichotomise treatment intensity from the National Minimum Wage and compare a "high-impact" and a "low-impact" group. While I follow their general approach of comparing sub-markets impacted to different degrees by the increases in UK minimum wages, I opt for using treatment intensity directly. In a recent methodological paper, Schmidheiny and Siegloch [2019] argue that "[T]he parameter estimates of such an artificial dichotomization are hard to interpret both in magnitude and direction. Moreover, the dichotomization of the treatment variable eliminates valuable information which could otherwise be used to identify the magnitude of the effect."

My main measure of treatment intensity is the ratio of the minimum wage to the median. Following the terminology of Machin and Manning [2004] and the Low Pay Commission, I refer to this variable as the minimum wage bite.[17] Figure 3 shows the evolution of the equivalent national-level measure, i.e. the ratio of the minimum wage to the *national* median.[18] Treatment intensity varies at the region-industry level and over time.

Regional measures of treatment intensity such as the share of workers earning a wage below the new minimum or the minimum wage relative to the mean or median wage are commonly used in the literature, especially in settings where the minimum wage is set at the national level [Stewart, 2002, Neumark and Wascher, 2004, Dolton et al., 2012, Caliendo et al., 2019]. Whilst a national minimum eliminates a source of variation present in settings with state- or province-level minimum wages such as the US and Canada, it is also more exogenous to region-specific shocks[19], which are a potential source of bias [Baskaya and Rubinstein, 2015].

As an alternative to this approach, workers slightly further up the wage distribution are often used as a control group [Arulampalam et al., 2004, Stewart, 2004a, Dickens et al., 2009, 2015, for example]. However, there is previous evidence of "spillover" effects on job amenities, which cannot be captured using such a strategy. Dickens et al. [2015] argue that concerns about spillover effects can be alleviated by using a group further up in the wage distribution, instead of those whose wages fall in a bracket immediately above the minimum wage. They acknowledge that there is a tension between the aims of choosing a control group that is as similar as possible to those directly affected, and avoiding spillover effects. There are, moreover, constraints on the availability of time-series data on job attributes at the individual level. This is why I focus on variation in

[17] It is sometimes referred to as the Kaitz index [Dolton et al., 2015]. This term is not unambiguous, as it is also used for the ratio of the minimum wage to the *mean* wage [Dolton et al., 2012], or for this ratio multiplied by the proportion of the labour force covered [Belman et al., 2015]. The term "bite", in other national contexts such as Germany, is sometimes used to refer to the proportion of workers paid at or below the minimum wage, but at least within the UK literature, it is used fairly consistently.

[18] I use publicly available median wages from the Annual Survey of Hours and Earnings. This leads to a slight difference with the figures on bite published by the Low Pay Commission, which are constructed using the median for the over-25s based on their own calculations with disaggregated data from the Annual Survey of Hours and Earnings.

[19] As Card et al. [1992] puts it, "From an evaluation perspective [...] a uniform minimum wage is an under-appreciated asset."

treatment intensity across geographical and industry groups.

In a robustness check for the introduction of the National Living Wage only (Section 3.3.3), I compare results for a second measure of treatment intensity, namely prospective coverage. This is the share of held by adults over 25 in 2015 which are paid below £7.20 and are therefore expected to be directly affected by the policy. This measure is highly, but not perfectly correlated with bite ($\rho = 0.94$). The bite measure has fewer missing values due to the ONS being able to estimate median wages with smaller coefficients of variation. However, in these smaller cells the estimated change in the outcome variables is also more uncertain. In an additional set of robustness checks, I use bite with respect to the 30th percentile, instead of the median.

If worker-friendly attributes are a normal good, regions and industries that are with high productivity growth may be expanding them at a faster rate, whilst at the same time experiencing faster wage growth which leads to a low bite. Similar endogeneity concerns have been raised in the literature on employment effects [Baskaya and Rubinstein, 2015]. I address this in a number of ways. I define the minimum wage bite with respect to the previous year's wage distribution. This is a more exogenous measure of each uprating's impact. Additionally, I condition on region-industry specific gross value added to capture productivity shocks, as Dolton et al. [2015] recommend as a remedy for a similar endogeneity concern in their analysis of employment effects of the National Minimum Wage.

In their previous work on the employment impact of the National Minimum Wage, Dolton et al. [2012] compared alternative measures of treatment intensity (the share at or below the National Minimum Wage and the spike). They concluded that they lead to qualitatively similar conclusions, suggesting that the choice among the different continuous treatment intensity measures is not a crucial one.

Baseline specification I estimate a fixed-effect regression with standard errors clustered by region-industry. The fixed effects capture differences in technology, which will affect the cost of providing different attributes.

The regression takes the following form

$$Y_{it} = X_{it}\beta_x + \beta_1 \text{bite}_{it} + \beta_2 \text{bite}_{it}^2 + c_i + \epsilon_{it}$$

where i indexes the region-industry cells and the outcome Y is the share of adult workers (according to the minimum wage structure in year t) who report the schedule type in question. The regional classification is on the basis of region of work. The covariate vector X includes the composition of the region-industry cell (or "cohort", as Deaton [1985] calls it) with respect to age and gender[20], non-UK nationality, educational attainment and presence of children in the family. In addition, I condition on regional employment and unemployment rates[21] and region-industry level gross value added, as well as the share of workers who are apprentices or young people, in the sense that they are not entitled to the adult rate of the National Minimum Wage, or to the National Living Wage. I provide summary statistics for these covariates in table 25. I also include a set of year dummies. All standard errors are clustered at the region-industry cell level and weighted by the number of underlying worker-level observations.

As mentioned above, I define the minimum wage bite with respect to the previous year's wage distribution, which is a plausibly more exogenous measure of impact than bite with respect to the contemporaneous distribution. Dolton et al. [2015] address the potential endogeneity problem by including regional gross value added, which I also do as an additional safeguard. I exclude the industry of agriculture, forestry and fishing. In addition to potentially important differences in the production process and issues with seasonal fluctuation, it is the smallest industry in terms of observations in the Labour Force Survey, which makes estimates of industry averages very noisy.

I allow for a non-linear effect of the bite on any given job attribute. This is likely to be important since most job attributes are naturally constrained - for example, an employer can discontinue a flexitime scheme, but they cannot offer negative flexitime. Therefore, employers wishing to reduce their provision of job attributes would switch to another job attribute when the margin of one has been exhausted. This would produce a pattern where a certain job attribute would be reduced in response to increases at a certain level of bite.

Dynamic specification Dolton et al. [2015] use a system GMM estimator to reflect the dynamic nature of employment. An estimate of employment effects of minimum wages in the UK is clearly vulnerable to simultaneity bias: Employment effects are a central concern for the Low Pay Commission in its recommendations, opening a clear

[20]using a set of dummies for young, middle-aged and older men and women, respectively, with the omitted category being middle-aged men

[21]from the Labour Force Survey, based on the unemployed workers' regions of residence

channel through which employment affects minimum wage increases. In contrast, whilst the Low Pay Commission does discuss some submissions from stakeholders concerned about a potential effect of the National Minimum Wage and National Living Wage on job attributes, this is far from a primary consideration for their recommendations. Therefore, my estimates are less likely to suffer from a reverse causality problem.

Nevertheless, job attributes may still be dynamic in the sense of a true state dependency, which the fixed-effect regression cannot properly account for. To address this concern, I also report results for Arellano-Bond (difference GMM) models. I estimate the models using a two-step procedure for efficiency reasons and adjust standard errors using the Windmeijer finite-sample correction [Windmeijer, 2005].

The main Arellano-Bond model takes the form

$$Y_{it} = \beta_1 \text{bite}_i t + \mathbf{x'_{it}}\beta + \delta y_{i,t-1} + c_i + \epsilon_{it}.$$

Analogously to the fixed-effects specification, I weight by the number of underlying worker observations. Using a separate instrument for each time period, variable, and lag distance leads to a problem of instrument proliferation, where endogenous variables are overfitted [Roodman, 2009]. I therefore restrict the number of lags used to construct the instrument to two and collapse the instrument set, using one instrument for each variable and lag distance. This leaves me with 35 instruments. In Section 3.3.3, I report some additional results where I cull the set of covariates to keep the instrument set manageable. In the main specification, I use the same set of covariates as in the fixed-effects regression, treating gross value added and the employment rate as predetermined covariates, and the remaining covariates as exogenous.

Dynamic effects are significant for some, but not all models I estimate. A single lag is sufficient to pass a second-order autocorrelation test for the vast majority of schedule types I examine and for some outcomes, the lagged value is not significant. This suggests that dynamics are not central to the determination of some of the schedule types analysed here. In the main part of this paper, I only report specifications with one lag. In Section 3.3.3, I report specifications with two lags for some schedule types.

Data and Descriptive Statistics

My approach requires me to identify region-industry cells where a higher impact of the minimum wage led to larger increases in low percentiles of the wage distribution. I use information on wages, job attributes and transitions from the Quarterly Labour Force Survey (LFS), supplemented with aggregate-level wage information from the Annual Survey of Hours and Earnings (ASHE), merged at the industry-region level.

Table 25: Summary statistics

	Unweighted		Weighted	
	Mean	Std. Dev.	Mean	Std. Dev.
NMW/NLW bite	0.604	0.145	0.575	0.140
Share of women	0.372	0.193	0.484	0.202
Age	44.21	2.631	43.75	1.734
Children aged 0-4	0.150	0.0414	0.152	0.0263
Children aged 5-9	0.154	0.0404	0.157	0.0248
Non-UK nationals	0.0691	0.0590	0.0809	0.0594
Degree or equivalent	0.232	0.124	0.291	0.138
GCE A level or eq	0.244	0.0833	0.223	0.0685
GCSE grades A*-C or eq	0.210	0.0590	0.203	0.0545
Share of youth	0.0599	0.0534	0.0584	0.0566
Share of Apprentices	0.00910	0.0116	0.00676	0.00834
Gross Value Added	0.953	0.105	0.938	0.0822
Unemployment Rate	0.0474	0.0122	0.0468	0.0118
Employment Rate	0.711	0.0323	0.718	0.0295
Observations	1296		1296	

National Living Wage/Minimum Wage bite is the ratio of the minimum wage to the the previous period's median wage in the region and industry. Weights represent the number of underlying individual-level observations.

The Labour Force Survey contains hourly *pay* and hourly *rate* data. Hourly pay is constructed from information on the last pay received, the time covered and hours worked per week. Since it is derived by combining information on these different aspects, there are multiple potential sources of measurement error [Skinner et al., 2002]. Hourly *rate* information is more accurate. But respondents who are not paid by the hour are not directly asked for an hourly rate, which means that we only observe it for a selective sample of just under half of observations with pay information. I therefore use external data on wages from the Annual Survey of Hours and Earnings at the region-industry level. This information is provided by employers, who are legally obliged to respond to the survey if their employee is selected for inclusion in the sample, and measurement error is less of a concern.

Table 26: Summary statistics for low- and high-bite cells

	Below-median bite		Above-median bite	
	Mean	Std. Dev.	Mean	Std. Dev.
NMW/NLW bite	0.488	0.0651	0.706	0.118
Share of women	0.486	0.230	0.482	0.147
Age	43.95	1.338	43.74	2.113
Children aged 0-4	0.154	0.0255	0.152	0.0255
Children aged 5-9	0.157	0.0214	0.156	0.0283
Non-UK nationals	0.0835	0.0600	0.0829	0.0605
Degree or equivalent	0.341	0.134	0.231	0.117
GCE A level or eq	0.216	0.0748	0.231	0.0497
GCSE grades A*-C or eq	0.181	0.0426	0.236	0.0545
Share of youth	0.0277	0.0174	0.103	0.0632
Share of Apprentices	0.00597	0.00800	0.00823	0.00789
Gross Value Added	0.937	0.0753	0.944	0.0828
Unemployment Rate	0.0489	0.0116	0.0457	0.0125
Employment Rate	0.720	0.0270	0.720	0.0285
Flexitime	0.129	0.0423	0.0933	0.0487
Other worker-led	0.0784	0.0670	0.0330	0.0401
Annualised hours	0.0492	0.0219	0.0486	0.0254
Zero hours contract	0.00705	0.00762	0.0138	0.0140
Unsocial hours	0.353	0.0674	0.494	0.137
∼ allowance	0.0429	0.0358	0.0201	0.0266
Observations	539		540	

National Living Wage/Minimum Wage bite is the ratio of the minimum wage to the the previous period's median wage in the region and industry. Both sets of statistics weighted by the number of underlying individual-level observations.

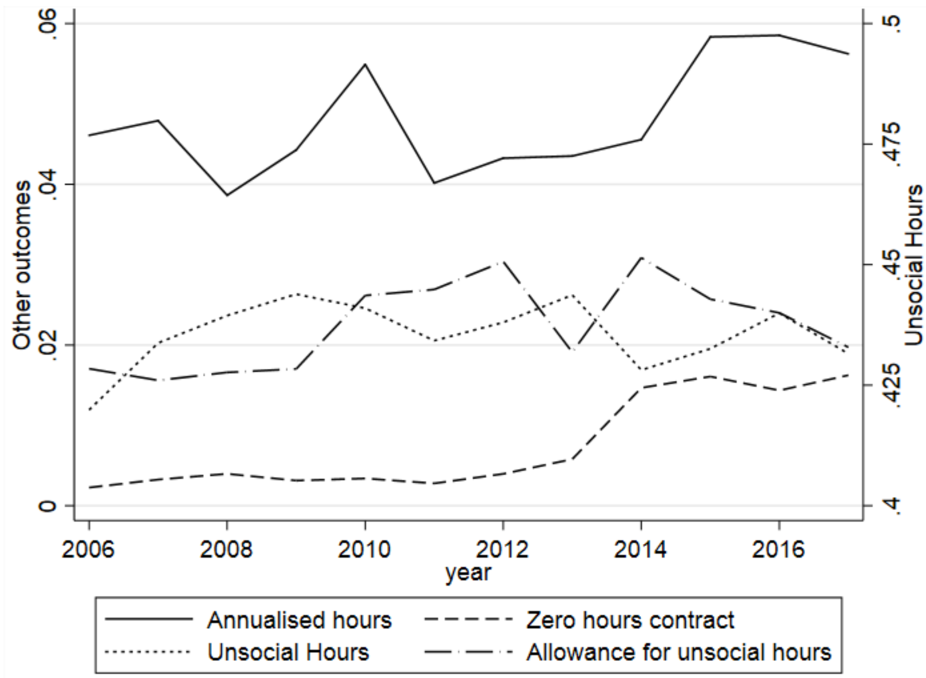
Regions are the eleven Government Office Regions of Great Britain[22]. Whilst ASHE recognises 21 industries, I aggregate these to the nine industries available in the Labour Force Survey, using averages weighted by the number of jobs in cases where several ASHE industries correspond to a single Labour Force Survey industry.

In the following sections, I will analyse whether regions and industries most affected

[22]Estimates for Northern Ireland are not available in disaggregated form by industry.

Figure 4: Employer-friendly types of schedule flexibility

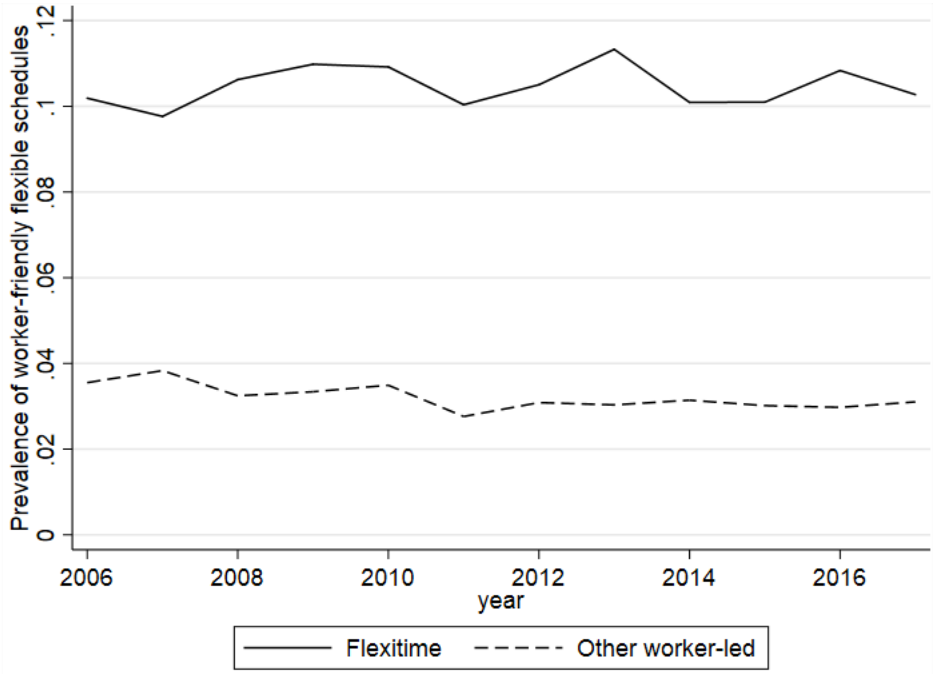
Average over region-industry cells, allowances conditional on working unsocial hours.



by the introduction of the National Living Wage saw changes in schedule amenities and disamenities. I consider four types of flexible or "non-standard" schedules: Flexitime schedules and other worker-led schedule types such as compressed work weeks, term-time working or job shares, are the two categories that are expected to primarily benefit workers. The remaining categories of zero hours contracts and annualised hours are expected to primarily benefit employers and represent a disamenity for workers. I also study effects on unsocial hours and allowances paid to compensate workers for them. I analyse the latter margin conditionally on the first, i.e. the two outcomes of interest are, firstly, working unsocial hours and secondly, receiving an unsocial hours allowance conditionally on working them.

On average across all cells, all schedule types fluctuated over the sample period (cf. figures 4 and 5), but only zero-hours contracts saw a dramatic change, with the share beginning to increase sharply in 2013. This increase may in part be driven by increased awareness and understanding of this type of schedule among respondents. However, there is no reason to believe that this increased awareness, driven by a national policy discussion and press coverage, should differ by treatment intensity of the

Figure 5: Worker-friendly types of flexible schedules, average over region-industry cells.



minimum wage. There is substantial variation across region-industry cells and there are cells in the sample which experienced increases and decreases of each schedule type examined.

Information on allowances for unsocial hours are part of the Labour Force Survey's pay section and therefore only available for a subset of the data, namely those respondents in the first or fifth quarter of their participation.

3.3.2 Results

Wages, Jobs and Employment Wages and employment effects of the National Minimum Wage have been studied extensively and are not the focus of this paper. The consensus in the literature is that the National Minimum Wage has had little or no effect on employment overall [Stewart, 2002, Metcalf, 2008, Dolton et al., 2012]. In something of an exception, Dickens et al. [2015] find a negative effect on employment retention for women working part-time. Dickson and Papps [2016] use a wage gap specification on data from the Annual Survey of Hours and Earnings, the Labour Force Survey and the British Household Panel Study from 1997-2013, defining treatment intensity as the amount by which workers' wages have to be raised to comply with the new minimum wage. They find a negative effect on turnover and on hours worked, but not on flows out of employment. The evidence found on flows into employment is mixed.

Table 27: Minimum wage bite and wage growth at the 30th percentile

	OLS		Fixed Effects		FEs with covariates	
NMW/NLW bite	1.296**	(0.553)	9.733***	(1.155)	9.466***	(1.188)
- squared	-0.794**	(0.400)	-3.829***	(0.874)	-3.678***	(0.896)
N	1060		1060		1060	

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Analytical weights. All include year fixed effects. (3) additionally includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices and region-industry level GVA.

Here, I limit myself to briefly showing effects on the sample I go on to use, to contextualise my results on job attributes. In order for results to be informative about the relationship between wages and job amenities, minimum wage increases need to induce wage growth, at least in the lower part of the distribution. Figure 3 plots the adult rate of the National Minimum Wage and National Living Wage in nominal and real terms as well as the minimum wage bite. Table 27 shows that a higher bite (with respect to the previous year's wage distribution) was associated with larger increases in the 30th wage percentile in the current year. Since I use the bite with respect to the lagged median wage, this does not imply an endogeneity problem.

Any model with a legal or natural lower bound on attributes (including the one sketched out in the appendix in Section 3.3.3) predicts that for some values of the minimum wage, some types of firms will become unprofitable and exit the market. This could happen alongside an adjustment in the attribute offer of the remaining firm, or it could be the *only* driver of changes in the attribute distribution in the market, with the remaining firms keeping their offer constant. So even though the literature has not found evidence of overall employment effects of minimum wages in the UK, an effect on non-wage amenities is not predicated on this (dis-)employment effect being zero.

To illustrate employment effects in this sample, I estimate regressions using the number of jobs from ASHE in a region-industry cell as an outcome. I find that a negative association between minimum wage bite and employment is significant in a least-squares regression, but not in a fixed-effects model, or a dynamic model (Table

Table 28: Minimum wage bite and jobs

	OLS		Fixed Effects		Arellano-Bond	
NMW/NLW bite	-2907.1**	(-2.88)	-138.8	(-1.32)	396.4	(0.72)
- squared	1306.6	(1.72)	76.07	(0.97)	562.3	(1.53)
Gross Value Added	65.78	(1.10)	91.95***	(7.95)	-11.03	(-0.18)
L.jobs					0.966***	(3.65)
Observations	990		990		862	
Av. Marg. Effect	-1334.3		-47.25		1055.6	
Autocorr. Coeff.					0.680	
Hansen Test					0.187	

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, share of youth and apprentices, region-industry level GVA and year fixed effects. 32 instruments used in (3).

28). This is in line with the results from Dolton et al. [2015], who use data for an overlapping but slightly earlier period. The dynamic specification for jobs is broadly analogous to the one used for the schedule outcomes. There are two differences, for obvious reasons: It is not weighted (because the outcome is not from the Labour Force Survey) and it does not include employment or unemployment rates as controls.

I also estimate a set of probit regressions using individual-level LFS data on inflows to and outflows from employment in the quarter preceding the survey date (Table 29). This means that the regressions capture flows just before as well as just before the each uprating of the National Minimum Wage/introduction of the National Living Wage. I assign individuals to their current industry for inflows, and to the industry of their last job for outflows and the outcome is a flow (i.e. a transition conditional on the previous state). I assign everyone to their region of residence rather than their region of work, since information on non-employed workers' previous region of work is incomplete. As in my main specification for schedule types, I allow for a quadratic effect to reflect possible switching between different margins of adjustment. I condition on region-industry level gross value added, as well as a range of personal characteristics and region and industry (but not joint) and year fixed effects.

None of the coefficients are significant at the 5% level. There is some evidence of a convex effect on inflows at a higher significance level (p=.08), with a positive but

Table 29: Minimum wage bite and inflows to and from employment

	Inflows		Outflows to non-participation		Outflows to unemployment	
Bite	-0.884	(0.542)	0.151	(0.357)	-0.0146	(0.287)
- squared	0.738*	(0.419)	0.0487	(0.301)	0.0426	(0.264)
Woman	-0.0647***	(0.0158)	0.0874***	(0.0152)	-0.0296**	(0.0120)
Children, 0-4	-0.0300	(0.0254)	-0.150***	(0.0367)	-0.115***	(0.0191)
Children, 5-9	0.0442*	(0.0247)	-0.103**	(0.0408)	-0.0811***	(0.0207)
Mother, 0-4 yo	-0.897***	(0.0390)	0.443***	(0.0448)	0.256***	(0.0291)
Mother, 5-9 yo	-0.297***	(0.0345)	0.114**	(0.0475)	0.158***	(0.0250)
Degree	0.596***	(0.0288)	-0.0886**	(0.0288)	-0.237***	(0.0225)
Higher education	0.475***	(0.0329)	-0.0372	(0.0335)	-0.168***	(0.0249)
GCE A level	0.341***	(0.0324)	-0.0214	(0.0299)	-0.140***	(0.0211)
GCSE A*-C	0.308***	(0.0231)	-0.0538*	(0.0293)	-0.102***	(0.0203)
Observations	108702		448963		448963	
P-value (bite)	0.212		0.501		0.975	

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Probit regression. NLW bite from ASHE, other variables from LFS. Includes year, region and industry fixed effects. P-value in table foot is for joint significance of linear and quadratic bite.

insignificant point estimate of the marginal effect at intermediate and high bites. This would be a plausible effect sign since higher wages could incentivise work, but the evidence is weak. Other coefficients behave as expected: mothers, especially those with children who have not yet reached school age, are significantly more likely to transition to un- or non-employment, with the opposite being true for fathers. Mothers are also less likely to transition into new employment. Young workers and less educated workers at every level of education are more likely to exit employment and less likely to enter it. There is no separate effect of gross value added, which is not too surprising after industry fixed effects and sorting of individuals with different characteristics has been taken into account.

In summary, consistently with the rest of the literature, there seems to be little or no effect on employment, especially once dynamics are taken into account. There is no evidence of an effect on outflows, and only weak evidence of a positive effect on inflows. I now move on to the main part of my analysis, focusing on changes in schedule amenities and disamenities.

Schedule disamenities Zero-hours contracts[23] and annualised hours are types of schedule flexibility that primarily benefit employers by allowing them to react to demand fluctuations. From the worker's perspective, a guaranteed number of weekly hours is a job amenity that (some) workers value, and I therefore a priori interpret zero-hours contracts and annualised hours as disamenities. I would therefore expect a higher minimum wage bite to be associated with a greater prevalence of these contract types.

In fixed-effects regressions, there is a significant convex effect on zero-hour contracts that is robust to a range of specifications (Table 30). The marginal effect is positive for the whole range of observed bites.

Using an Arellano-Bond specification, a single lag is enough to pass an autocorrelation test. The lagged prevalence of zero-hours contracts is not significant, which is perhaps intuitive given that the advantage of these contracts is their flexibility. The marginal effect of the minimum wage bite remains significant at the 5% level for bite values of 60% (this is very close to the mean bite) and above. Hansen tests for overidentification are not rejected. Roodman [2009] highlights a p-value close to 1 as a warning sign that the test is weakened by too many instruments. The value of .74 is not very close to 1, but it is in a range where he recommends caution nonetheless. Reassuringly, the specification does not reject a Sargan test, which is not weakened by many instruments.

In Section 3.3.3, I also present some alternative specifications which achieve better values of the Hansen test. The sign and significance of both coefficients remains the same, although their point estimates change somewhat. In summary, there is strong evidence that a higher minimum wage bite has been associated with an accelerated expansion of this schedule type for higher-bite industries.

Annualised hours are another type of flexibility that are primarily driven by employer needs[24]. There are no significant effects on the prevalence of annualised hours in any

[23] Workers on zero-hours contracts are on call, without any guaranteed hours. Petkova [2018] reports that workers on zero-hours contracts have more variable working hours and are more likely to report wanting more hours than they currently have.

[24] According to the UK employment arbitration body ACAS, "[i]n an annual hours system an employee works a certain number of hours over the whole year, but with a certain degree of flexibility about when those hours are worked. Normally, a period of regular hours or shifts forms the core of the arrangement, with the remaining time left unallocated and used on an 'as needed' basis. Sometimes, the employee is paid in advance for the unallocated time, and may be called upon at short notice, perhaps to cover colleagues or according to a surge in demand. Annualised hours are most often used for shift workers, but in theory they can be applied to any employee. [...] In manufacturing, they are sometimes used to achieve continuous production throughout the year. Organisations that need to run 24 hours a day all

Table 30: Effect of minimum wages on zero-hours contracts and annualised hours, fixed-effects and dynamic specifications

	Zero-Hours Contracts		Annualised Hours	
	(1)	(2)	(3)	(4)
	FE	Arellano-Bond	FE	Arellano-Bond
NMW/NLW bite	-0.213***	-0.106	0.0352	-0.380
	(0.0560)	(0.156)	(0.121)	(0.444)
- squared	0.221***	0.261**	0.0362	0.0801
	(0.0464)	(0.0944)	(0.104)	(0.233)
Graduate Share	0.00927	0.0241	-0.0385	-0.0285
	(0.0125)	(0.0154)	(0.0305)	(0.0812)
Gross Value Added	-0.00240	-0.0111	-0.00193	0.0247
	(0.00767)	(0.00926)	(0.0172)	(0.0361)
Employment Rate	-0.0520	-0.0681	0.00125	0.313*
	(0.0329)	(0.0673)	(0.100)	(0.180)
L.Outcome		0.0793		0.316**
		(0.117)		(0.0995)
Constant	0.0798**	0.0142	0.0381	0.0446
	(0.0322)	(0.0699)	(0.0783)	(0.303)
Observations	966	966	966	966
Av. Marg. Effect	0.0396	0.193	0.0767	-0.289
Autocorr. Coeff.		0.626		0.902
Hansen Test		0.744		0.161

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW bite from ASHE, other variables from LFS. Analytical weights. Includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices, region-industry level GVA and year fixed effects. 35 instruments used in (2) and (4).

specification (Table 30). An Arellano-Bond model with one lag passes an autocorrelation test and there is significant persistence in the share of annualised hours. It seems likely that introducing or removing a system of annualised hours requires significant changes to the production process and that it would therefore be a less appropriate margin for short-term adjustments.

There is a significant concave effect of a higher minimum wage bite on the prevalence of unsocial hours (Table 31). Unsocial hours are defined here as working evenings, nights

year, such as hospitals and the emergency services, can also find this arrangement beneficial." [ACAS, 2019]

Table 31: Effect of minimum wages on unsocial hours and allowances for them, fixed-effects and dynamic specifications

	Unsocial Hours		Allowances for Unsocial Hours		Saturday hours only	
	FE	A.-Bond	FE	A.-Bond	FE	A.-Bond
NMW/NLW bite	0.642***	0.658	0.130	-0.253	0.769***	0.201
	(0.179)	(0.539)	(0.0951)	(0.482)	(0.159)	(0.402)
- squared	-0.221	-0.0395	-0.0515	0.151	-0.375**	-0.246
	(0.157)	(0.258)	(0.0678)	(0.154)	(0.132)	(0.192)
Graduate Share	-0.147**	-0.132	-0.0515	-0.0275	-0.232***	-0.248***
	(0.0436)	(0.0862)	(0.0398)	(0.0592)	(0.0432)	(0.0697)
Gross Value Added	0.0832***	0.115**	0.0252*	0.0400	0.0828***	0.0443
	(0.0226)	(0.0410)	(0.0128)	(0.0346)	(0.0205)	(0.0276)
Empl. Rate	0.281**	0.152	0.116	0.324	0.128	0.420*
	(0.135)	(0.260)	(0.123)	(0.255)	(0.123)	(0.242)
L.Outcome		0.0693		0.0594		0.230***
		(0.0530)		(0.0738)		(0.0698)
L2.Saturday work						0.157**
						(0.0506)
Constant	-0.0649	-0.130	-0.120	-0.182	-0.121	0
	(0.107)	(0.331)	(0.0951)	(0.317)	(0.102)	(.)
Observations	966	966	966	966	966	879
Av. Marg. Effect	0.389	0.613	0.0708	-0.0798	0.339	-0.0811
Autocorr. Coeff.		0.465		0.768		0.170
Hansen Test		0.181		0.474		0.0512
Number of instr.		35		35		27

FE: Fixed-effects model, A.-Bond: Arellano-Bond model. Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW bite from ASHE, other variables from LFS. Analytical weights. Saturday work is a subcategory of unsocial hours. Includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices, region-industry level GVA and year fixed effects.

or weekends. This is in line with theoretical predictions for a job attribute that generates disutility for workers. Presumably, switching to more sociable hours would be costly for employers, otherwise they would have no reason not to do so. In an Arellano-Bond model, one lag is sufficient to pass the autocorrelation test, and there is significant persistence. However, the effect of the bite is no longer significant once this persistence is accounted for.

Drilling a little deeper, the effect in the fixed-effects regression is driven by Saturday work, rather than work on Sundays, nights or evenings. The main Arellano-Bond specification for Saturday work rejects the Hansen test, as do many of the adjustments used in other parts of the paper. I report a specification with two lags and a reduced covariate vector[25]. The coefficients on bite are not significant. The same is true for the main Arellano-Bond specification, if one is prepared to ignore the overidentification issue. A potential problem is that I can only observe the extensive margin, i.e. I do not observe how common Saturday work is for those workers who report it. More than one in four workers (weighted average across cells) report working Saturdays, but they may have varying degrees of control over this decision. Heterogeneity between frequent and occasional Saturday workers as well as between (more) voluntary and involuntary ones, could contribute to the large but imprecisely estimated coefficients for unsocial hours in general, and Saturday work in particular.

There is no evidence in any of the specifications of an effect on allowances for unsocial hours, conditional on working such hours (Table 31). It is worth bearing in mind that this information is only available for a subset of the data and any effect would therefore be more difficult to detect than for the other outcomes.

Schedule amenities There is a significant negative (somewhat convex) effect of the minimum wage bite on the prevalence of flexitime schemes (Table 32). This is entirely driven by jobs in public administration, education and health. This is the largest industry which gives the corresponding cells a high weight and it has seen large increases in the minimum wage bite alongside an almost 20% decrease in the share of workers reporting flexitime work over the sample period.

Flexitime is a schedule type with clear benefits for workers and a potential productivity cost if production relies on complementarities between workers. The negative impact of the minimum wage bite on flexitime is therefore consistent with theoretical predictions. In an Arellano-Bond model, flexitime is persistent, which is intuitively plausible. The linear negative effect of the bite remains significant, and the marginal effect of the bite even becomes much larger. Neither the Hansen nor the Sargan test are rejected, and the Hansen test gives no indication of a problem of instrument proliferation.

It is worth bearing in mind that flexitime is more prevalent among higher-paid

[25] including education dummies, the youth and apprentice shares, gross value added, (un-)employment rates and a single dummy for the period after 2010. This cut-off was chosen because it provided the highest R-squared out of a set of regressions, one for every year in the sample.

Table 32: Effect of minimum wages on flexitime and other worker-led flexible schedules

	Flexitime		Other Worker-Led	
	FE	Arellano-Bond	FE	Arellano-Bond
NMW/NLW bite	-0.408**	-0.917*	-0.209**	-0.372
	(0.147)	(0.532)	(0.0739)	(0.279)
- squared	0.266**	0.417	0.166**	0.294**
	(0.116)	(0.259)	(0.0557)	(0.0981)
Graduate Share	0.0662	0.0474	-0.0149	0.0275
	(0.0421)	(0.0839)	(0.0181)	(0.0418)
Gross Value Added	0.00761	0.0655	0.0324**	0.0390**
	(0.0191)	(0.0430)	(0.00955)	(0.0182)
Employment Rate	-0.0240	0.151	0.0134	0.156
	(0.117)	(0.234)	(0.0675)	(0.182)
L.Outcome		0.225**		0.400**
		(0.0808)		(0.150)
Constant	0.188*	0.266	0.105**	0.0326
	(0.113)	(0.239)	(0.0460)	(0.134)
Observations	966	966	966	966
Av. Marg. Effect	-0.103	-0.440	-0.0189	-0.0350
Autocorr. Coeff.		0.196		0.664
Hansen Test		0.254		0.0409

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW bite from ASHE, other variables from LFS. Analytical weights. Includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices, region-industry level GVA and year fixed effects. 35 instruments each in columns 2 and 4.

workers (using LFS earnings data as-is). Any effect is therefore unlikely to be driven by direct effects on workers whose wages had to be raised to comply with the new minimum wage. Spillover effects on workers who were already earning more than the new minimum wage are a more likely channel. These spillover effects would arise if employers seeking to compensate for the minimum wage increase reduce, or fail to increase, the value of their non-treated workers' total compensation package. In Section 3.3.3, I present some additional evidence that the effect on flexitime is weaker when the treatment is re-defined with respect to the 30th percentile instead of the median. I also discuss some of the reasons for this divergence.

There is a smaller effect on worker-led flexible schedules other than flexitime which is significantly negative at low bites and becomes significantly *positive* at very high bites. Workers are likely to view these schedule types - a compressed work week, job sharing, term-time working - as an amenity and they could potentially be very important for combining work and caring responsibilities. However, they vary in their cost to the employer and the groups of workers in each individual schedule type are too small to analyse them all separately. The effect is driven by the large industry of public administration, education and health, where these schedule types are much more common than in any other industry. The heterogeneous effect could also be driven by different effects on directly treated workers and those affected by spillovers.

The Arellano-Bond model is once again able to pass an autocorrelation test after inclusion of just one lag. There is significant persistence, which is consistent with many of these schedule types requiring a substantial investment to set up. The quadratic effect of the bite remains significant.

In summary, while there is some evidence for a decreasing effect of a high minimum wage bite on the prevalence of schedule amenities, the picture is more nuanced than for disamenities. Heterogeneity across different schedule types appears to be important, as is the measure of the minimum wage bite used, and the effect is primarily driven by a single industry, namely education, health and public administration.

3.3.3 Extensions and Robustness Checks

Table 33: Share of women in industry-region cells, by industry across all years

	Mean	Minimum	Maximum
Energy and water	0.216	0	0.372
Manufacturing	0.257	0.180	0.374
Construction	0.121	0.0426	0.184
Distribution, hotels and restaurants	0.518	0.406	0.601
Transport and communication	0.238	0.165	0.318
Banking and finance	0.471	0.386	0.534
Public admin, education and health	0.719	0.650	0.756
Other services	0.551	0.437	0.716
Total	0.386	0	0.756
N	1,152		

Female-majority industries Both the literatures on workers' valuation of schedule amenities and on minimum wages emphasise gender as an important dimension of worker heterogeneity. Below, I examine the particular role of female-majority industries in the effects previously described.

Unsurprisingly, variation in the share of women across industry-region-year cells is strongly associated with the industry dimension (Table 33), whereas variation across regions and over time is very limited. For many of the schedule types analysed, the impact is strongest on female-majority industries, including when those industries do not have a particularly high minimum wage bite. However, the dynamics of female and male-majority industries are in many cases not very well captured by the Arellano-Bond models, several of which fail overidentification tests.

For instance, in the case of zero-hours contracts, the average marginal effect of a high minimum wage bite is significantly larger for female-majority industry-region cells. This is because the estimated marginal effect is consistently positive for these industries. In contrast, for male-majority industry-regions, the effect is negative at very low bites and then rapidly increases, surpassing the marginal effect for female-majority industry-regions at very high bites.

As with the main specification, there is not much evidence of an effect on annualised hours. The coefficients for female-majority industry-regions are only significant at the 10% level, and for the male-majority industry-regions, not at all. If there is any effect, it would be a positive effect in female-majority industries, but the evidence is weak.

In the case of unsocial hours, the patterns from the main specification are once again confirmed in the fixed-effects regression, with the results pointing to a larger effect in female-majority industry-regions. There is no effect on allowances for unsocial hours conditional on working those hours. The dynamic specification, with any choice of lags, year dummies and either the standard or a reduced covariate vector, again fails the Hansen test. It does confirm the pattern – a positive effect that is larger in female-majority industries.

Looking at schedule amenities, I previously set out that the result that the negative effect on flexitime is driven by the large industry of public administration, education and health, an industry with a high share of women workers. In line with this, the effect of a higher minimum wage bite is only significant in industry-region cells with a majority of women workers (Table 37). The pattern persists in the dynamic specification.

This specification also sheds some additional light on the non-linear effect found on other worker-led schedule types: Life for flexitime, the effect is only significant in female-majority industry-region pairs and the average marginal effect, which was zero in the baseline specification, is negative for female-majority cells.

As discussed in the main part of the paper, these schedule types are highly persistent and it is plausible that at least some of them would require a substantial organisational effort to introduce. The dynamic evolution in industries with and without a female majority among workers is not easy to capture: The baseline specification as well as several of the alternatives used elsewhere in the paper (fewer time dummies, different versions of the covariate vector or additional lags) all fail the Hansen and Sargan overidentification tests. In summary, the evidence of a negative effect on schedule amenities values by workers is stronger in female-majority industry-region cells, but the dynamics of worker-led types of flexibility other than flexitime are not very well captured by the model.

Alternative dynamic specifications for zero-hours contracts In this section, I will address some concerns with the dynamic specification for individual schedule types. For zero-hours contracts, the Hansen test in the specification reported above gives a p-value of .75, which Roodman [2009] considers a value where caution about the possibility of instrument proliferation is needed. Even though the Sargan test, which is not weakened by many instruments, gives no additional reason for concern, I present two alternative specifications in table 38. The coefficients' sign and significance are preserved in both specifications.

In the first one, I include a second lag, which brings the Hansen p-value down to .10. Given that none of the lagged values are significant, this is not my preferred specification. For the second specification, I cull the covariate vector to only include gross value added and the employment rate as predetermined covariates and the graduate, youth and apprentice shares as independent covariates. I also replace the full set of year dummies with a single dummy for the period after 2013. This cut-off marks a visual break in the time-series and also offers the greatest explanatory power in a linear regression. This only reduces the p-value of the Hansen test to .48, but the number of instruments is down to just 15, which should also alleviate concerns about instrument proliferation.

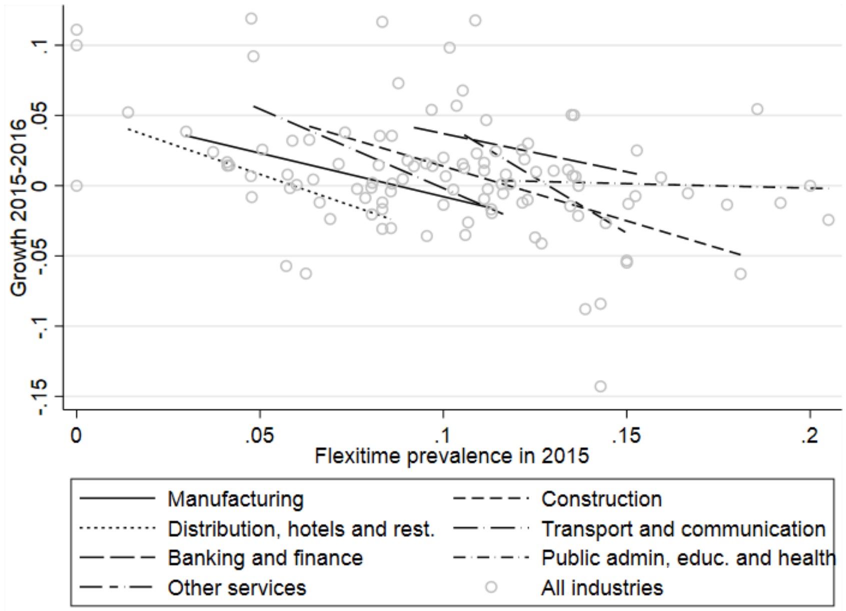
Pre-coverage and the National Living Wage introduction The National Living Wage was announced in July 2015. The BBC called the announcement "[o]ne of the George Osborne's big surprises in his budget". A new legal wage minimum of £7.20 applied to workers aged 25 or over from April 2016, less than a year after the initial announcement. This represented an increase of 7.5% compared to the 2015 adult rate of the National Minimum Wage. There was no change for workers between the ages of 21 and 25, who were entitled to the adult rate of the National Minimum Wage, but not the National Living Wage.

This was by far the largest minimum wage increase in the sample. For this change, I am able to introduce an additional measure of treatment intensity, namely the anticipated coverage, or the share of workers over 25 whose wage in 2015 was below the introductory rate of £7.20. This data is not generally available at the region-industry level, which is why I am unable to use it on all minimum wage increases in the sample. This measure is highly, but not perfectly correlated with the National Living Wage's bite at introduction ($\rho = 0.94$). Comparing the two alternative measures is interesting for two reasons: Firstly, if both are measures of impact, they can serve as a robustness check for each other and results which hold with both measures are more credible than ones which only hold for one of them. However, there could also be genuine differences in the impact on high-bite versus high-coverage regions and industries: High-bite but low-coverage regions and industries would be strongly affected by spillover effects on those (by construction, many) workers who earn between the introductory National Living Wage rate and the median.

The sample is restricted to the two years surrounding the introduction, and additionally by using anticipated coverage as a measure of treatment intensity, which has more missing values than the bite measure. I present two specifications on the effect of the National Living Wage's introduction. The fixed-effect regression used in the main part of the paper produces no significant effects at the 5% level for any of the outcomes analysed (table 39). Only the positive quadratic effect on unsocial hours is significant at the 10% level, predicting a U-shape, in contrast to the results for the whole period in the main section, which predicted a positive relationship. However, when London is excluded, the point estimate of the average marginal effect switches sign and the coefficient loses significance. In summary, this specification does not deliver evidence of a widespread effect of this single minimum wage increase on unsocial hours.

To limit the number of parameters to be estimated, I also present a specification

Figure 6: At the time of the National Living Wage introduction, flexitime plateaued or decreased in areas with high previous prevalence levels.



with a reduced set of covariates, a linear effect of coverage and separate industry and region fixed effects, instead of the industry-region fixed effects used in the main part of the paper (table 40). Contrary to expectations and the main set of results, a positive effect on flexitime is significant at the 6% level. The context for this result is that cells which had recorded a high level of flexitime in 2015, saw stagnation or a decline in 2016. The pattern is present within industries (cf. figure 6). Since fixed effects account for this, the effect disappears in the main specification.

Bite with respect to the 30th percentile Minimum wage bites are commonly defined with respect to the median. However, it is possible that the lower part of an industry's wage distribution is more relevant to the impact that a minimum wage increase has. If there is a lot of dispersion in the middle of the distribution, a low bite may also mask a large mass of affected workers at the bottom. To address this possibility, I present versions of both main specifications (fixed effects and Arellano-Bond models) using bite with respect to the 30th percentile as a measure of treatment intensity in table 42. As expected, the two bite measures are highly correlated with each other ($\rho = .977$).

Fixed-effects results for the schedule disamenities confirm the results in section 3.3.2.

Similarly to the main results, effects on annualised hours and allowances for unsocial hours are not significant in either a fixed-effects or an Arellano-Bond specification, and regressions for these two outcomes are not shown here.

The dynamic specification for zero-hours contracts broadly confirms the result for the original definition of the bite: Both the linear and the quadratic coefficient are highly significant and the average marginal effect is positive, with a large and significant positive marginal effect at very high bites.

As with the standard definition of bite, there is a positive and significant effect in the fixed-effects regression on unsocial hours that does not carry over to the dynamic specification, although the average marginal effect is still negative. The original Arellano-Bond specification using the newly defined bite fails the Sargan and Hansen overidentification tests. The overidentification issue is not a straightforward one to resolve. An amended specification using a reduced covariate vector[26], a single time dummy for the period after 2009 and with two lags does pass both tests and is the one I report in table 41. No other permutation of these alterations to the original specification produces significant coefficients on the newly defined bite, and nor does the original specification itself.

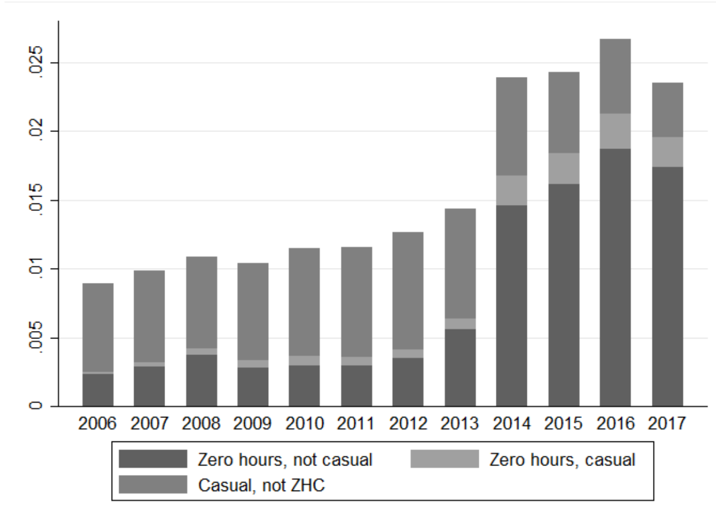
For amenities, the coefficients in the fixed-effects regression preserve their sign, but are substantially smaller than the main result and only significant at the 10% level. As discussed in section 3.3.2, the effect of minimum wage bite with respect to the median is driven by the large industry of public administration, education and health. Compared to other industries, this gap between bite at the median and bite at the 30th percentile is negative, i.e. this industry has relatively low bite at the median given its bite at the 30th percentile.

This is apparent from visually inspecting a scatter plot and also in a simple OLS regression of bite at the median on bite at the 30th percentile interacted with a dummy for public administration, education and health: The interaction effect is highly significantly negative. Differences in bite of course track differences in the shape of the wage distribution by construction: Public administration, education and health has the largest gap between the 30th percentile and the median of all industries in the sample, at just over £3. This places the industry next to a different set of comparator industries in both regressions, leading to the weakening of the result.

The marginal effect on other worker-led schedules is now negative across nearly all

[26]with gross value added and the employment rate as predetermined covariates and the graduate, youth and apprentice shares as independent covariates

Figure 7: Sample share of zero-hours contracts and casual work over time



of the observed bites[27] and significant at low and intermediate bites, instead of the U-shaped effect previously found. The original dynamic specification for other worker-led schedules using bite based on the 30th percentile also fails the Hansen and Sargan over-identification tests. I therefore report an alternative specification with a reduced covariate vector (analogous to the one used for unsocial hours above) with two lags, which passes both tests for both schedule amenities. For a number of variations on the original dynamic specification, whether or not they pass the Hansen test, the linear coefficient on the newly defined bite is negative and highly significant. The average marginal effect is also negative, as in the main results.

Zero Hours Contracts versus "casual" work Unlike other outcomes analysed here, zero-hours contracts could be considered to be a job security disamenity as well as a schedule disamenity, since not only the distribution of hours across time, but also the total number of hours is uncertain and could potentially drop down to zero. This dimension could be as or even more important for workers' utility as the schedule dimension. Different employers may also use zero hours contracts for different purposes, with the result that the expected trajectory of a job may be more akin to a job with a regular schedule but uncertain duration, or an unpredictable schedule but an expected long duration.

[27] up to 95% for the fixed-effects regression and everywhere for the Arellano-Bond specification

From a policy perspective, we would be interested in whether increased use of zero-hours contracts induces a "casualisation" of work. Figure 7 shows that a minority of zero-hours contract workers characterise their employment as "casual". In addition, there are overlaps between zero-hours contracts and other types of non-permanent work including agency work, contracts for a specific task and "other" reasons for non-permanency. These account for a smaller share of zero-hours contract work, and considering all of them together, they still account for a minority of zero-hours contract jobs. The large increases in casual *and* zero-hours contract work and zero-hours, *non*-casual work have have only been accompanied by small decreases in other forms of casual work.

Estimating the main specifications on the share of workers whose job is either casual *or* a zero-hours contract confirms the previous results for zero hours contracts only. There is some evidence of a decrease in casual work that is not on a zero-hours contract, but it is not robust to accounting for dynamics using the Arellano-Bond specification. Taken together, this implies that the increased prevalence of zero-hours contracts at high minimum wage bites is not solely due to a sustitution from other types of casual work, although this may play a partial role.

The response that a job is non-permanent for "other" reasons could also reflect the perceived risk of a zero-hours contract employer implicitly terminating the employment relationship by no longer giving the worker any hours. However, there is no effect of minimum wage increases on jobs that are on-permanent for "other" reasons *and* a zero-hours contract. In fact, the increase in zero-hours contracts is entirely in those jobs that employees describe as permanent, where coefficients are similar in size and significance to the specification for zero-hours contracts in general described above. I include a second lag in the Arellano-Bond specification since the specification with one lag fails the Hansen test. The potential issue of the Hansen test failing due to many instruments is addressed earlier in this section. In summary, whilst the effective job security in the additional zero-hours contract jobs associated with minimum wage increases may still be poor, the results nevertheless suggests that the effect of minimum wages on zero-hours contracts has primarily affected them as a schedule type, rather than as a form of casual or temporary contract.

Two-way clustered standard errors The main qualitative results (a negative effect on flexitime and other worker-friendly types of flexibility and a positive effect on zero-hours contracts) are robust to two-way clustered standard errors. Results for this specification are shown in Table 45.

Conclusion

Twenty years after the introduction of the National Minimum Wage in the UK, the consensus view is that it has had no effect on employment. I have used region-industry-level data to analyse whether employers have instead adjusted schedule amenities and disamenities in response to increases in minimum wages in the UK. This alternative adjustment channel could have important implications for the distributive consequences of minimum wages for workers.

Using fixed-effects regressions and dynamic models, I have shown that in regions and industries highly impacted by minimum wage increases, schedule disamenities such as zero-hours contracts and unsocial hours have increased in prevalence. Annualised hours, on the other hand, have not reacted, likely because they require substantial changes to workplace organisation. Schedule amenities valued by workers present a more complex picture. In accordance with theoretical predictions, flexitime has decreased in prevalence where the impact of minimum wage increases has grown. On the other hand, minimum wage increased have a non-linear relationship with other worker-led schedule types, including compressed work weeks, job shares and term-time working. This is likely due to differences in costs of adjusting to and sustaining such schedules.

Appendix I: Job attributes and a minimum wage in a search model

To fix ideas, consider the following equilibrium search model based on Hwang et al. [1998][28] where jobs are characterised by a wage w and an attribute x. A worker employed in a job *(w,x)* receives utility $v(w,x)=w+h(x)$. For the sake of conciseness, fix $h'(x) > 0$, i.e. define the attribute as a worker-friendly amenity, or absence of a burden. I follow Hwang et al. [1998] in assuming that nature constrains x to be non-negative. A firm offering a bundle valued at v will have size $m(v)$ in equilibrium.

Firms differ in their cost of providing the amenity.[29] For ease of exposition, without a minimum wage, assume there are 2 types of firms operating in the market. The cost of

[28]The worker side of this model was essentially set out in Gronberg and Reed [1994], however, that paper is not very explicit when it comes to firms' decision-making process on the composition of job offer bundles, which is the most relevant decision margin for this application.

[29]In some cases, the cost of the attribute will be a direct monetary cost, such as in the case of a bonus. In others, it will be the cost of mitigating a disamenity, for example through health and safety investments. In still others, it will be lost product, for example if by introducing flexible schedules, a firm forgoes gains from synchronised working.

providing amenity x for a firm of type j is $c_j(x)$, with $c_1 < c_2\ \forall x > 0$ and $c(\cdot)$ fulfilling regularity conditions, $c_j(0) = c_j'(0) = 0$ and $c_j''(x) > 0$. All firms produce the same output ρ per worker gross of attribute provision costs.

In choosing the job offer to make, a firm maximises profits $[\rho - w - c_j(x)]m[u(w,x)]$. If unconstrained, a firm of type j will choose to offer attribute level $x_j^\star$ such that $c_j'(x_j^\star) = h'(x_j^\star)$. Type-1 firms will therefore choose to provide a higher level of the attribute than type-2 firms. Given this choice, there is then a range of feasible wages, trading off profit-per-worker and firm size. In equilibrium, no firm will make an offer that will always be rejected. Therefore, every firm will at least offer the unemployed worker's reservation utility $v^\star$. The lowest wage offered is $\underline{w}_2$, characterised by $v(\underline{w}_2, x_2^\star) = v^\star$. All firms of the same type earn the same profits in equilibrium, and the distribution of utilities form job bundles offered will be continuous.

Now consider the introduction of a minimum wage w^{min}. Assume for now that the level of the minimum wage is such that $v(w^{min}, 0) < v_{new}^\star$ (I will discuss the other case below). Denote by x^{min} the level of the attribute that, combined with the minimum wage, delivers the reservation utility of an unemployed job seeker, i.e. $v(x^{min}, w^{min}) = v^\star$. Van den Berg [2003] provides conditions on the search environment that ensure that an equilibrium with a low reservation wage that enables the survival of type-2 firms exists.

Case I: Type-2 firms are constrained in their wage offers, but succeed in adjusting the attribute If $\rho > c_2(x^{min}) + w^{min}$ but $v(w^{min}, x_2^\star) > v^\star$, firms of type 2 will still be profitable. However, the preferred bundle of the lowest-paying firm, $(x_2^\star, \underline{w}_2)$, is no longer feasible, as the wage has to comply with the new statutory minimum. Given this wage increase relative to the unconstrained response to $v^\star$, this firm has an incentive to decrease the quantity of x it offers, since this will cut costs without affecting firm size. As in the unconstrained situation, they cannot increase their size by poaching workers from any other firm, so they have no incentive to offer a utility level beyond $v^\star$. They will thus offer the bundle (x^{min}, w^{min}). Profits will still be lower than at their preferred bundle and the condition $c_2'(x) = h'(x)$ will no longer hold. In equilibrium, all firms of the same type must still earn the same profits, and the distribution of utilities must still be continuous. Given that at the lower bound of the distribution $c_2'(x) < h'(x)$, a firm that aims to offer a marginally higher utility than $v^\star$ will prefer to increase the attribute rather than the wage, since a marginal increase in utility can be delivered at cost 1 through the wage, but at cost $c_2'(x)/h'(x) < 1$ through the attribute. As long as

$x < x_2^\star$, firms offering better bundles will do so via an improved attribute. Unlike in the unconstrained equilibrium, there will therefore be a non-degenerate distribution of attribute provision within a single type of firm. If $v(x_2^\star, w^{min})$ is less than $\underline{v}_1$ (the lower bound on the distribution of utilities offered by type-1 firms and at the same time, the upper bound on the offer by type-2 firms), all firms offering a better-valued job bundle than $(x_2^\star, w^{min})$ will continue to offer $x_2^\star$ and increase the wage.

Since there is no change in the number of firms operating in the market, there will be no change in offer arrival rates. As long as there is enough scope for a decrease in x to compensate for the higher wage induced by the minimum wage, the offer distribution *F(v)* chosen by firms will remain the same. In turn, workers will have no reason to change their offer acceptance strategy, since the offer distribution in terms of utilities remains the same.

Case II: Type-2 firms are constrained in both their wage and attribute offers If $v(w^{min}, 0) > v^\star$, the distribution is bounded below by $v(w^{min}, 0)$ instead of by $v^\star$. There will once again be a non-degenerate distribution of x: The lowest-utility offer will be $v(w^{min}, 0)$ and subsequent firms will prefer to increase utility by offering a better attribute until $x_2^\star$ is reached. The whole distribution of utilities offered will be shifted upward relative to the unconstrained case.

Both in this case and the previous one, we should observe a proportion of firms lowering their attribute offer. They should be firms for whom the minimum wage is binding.

Case III: Type-2 firms become unprofitable If $\rho < c_2(x^{min}) - w^{min}$, or $\rho < w^{min}$ in the case where no x^{min} exists, then all firms of type 2 will cease to be profitable and exit the market. If $v(x_1^\star, w^{min}) < v^\star$, then type-1 firms will choose the same attribute level as before and the situation is analogous to the one without a job attribute analysed by Van den Berg [2003]. He shows that under certain assumptions on the matching function and the search parameters, the wage distribution as a whole will shift upwards. By re-parametrising $\rho - c_j(x_j^\star) = p_j,\ j = 1, 2$, his conclusions apply. In this situation, the composition of attributes in the market changes, but this is purely due to type-2 firms, which provided a lower level of the attribute, exiting the market. No firm changes their attribute offer.

The cases where the minimum wage is set so high as to constrain type-1 firms are essentially analogous. In summary, depending on the firm's cost of providing the job attribute and the level of the minimum wage, effects on the provision of job attributes in the market could be positive or negative. Negative effects would arise through changes within-firm, whereas positive effects could only arise through selection, i.e. exit of firms providing lower levels of attributes. This would likely be associated with some unemployment, at least in the short run while workers reallocate to the more productive firms. Most of the previous empirical literature has found no effect on employment of the National Minimum Wage [Stewart, 2002, 2004a,b, Metcalf, 2008, Dolton et al., 2015], although there are some exceptions that find negative effects for specific groups of workers [Dickens et al., 2015, women part-time workers] or small positive effects for specific periods [Dolton et al., 2012]. I would therefore not expect to find positive effects.

Table 34: Effect of minimum wages on zero-hours contracts and annualised hours, female-majority vs male-majority industries

	Zero-Hours Contracts		Annualised Hours	
	FE	Arellano-Bond	FE	Arellano-Bond
NMW/NLW bite	-0.291***	-0.308*	0.156	0.421
	(0.0522)	(0.164)	(0.116)	(0.260)
- squared	0.276***	0.393**	-0.0583	-0.124
	(0.0395)	(0.135)	(0.0928)	(0.198)
Female majority	-0.0955***	-0.199**	0.124*	0.305**
	(0.0258)	(0.0644)	(0.0641)	(0.106)
Female majority	0.252**	0.578**	-0.354*	-0.843**
× Bite	(0.0768)	(0.200)	(0.195)	(0.330)
Female majority	-0.165**	-0.416**	0.256*	0.569**
× Bite squared	(0.0566)	(0.148)	(0.145)	(0.247)
Graduate share	0.000591	0.0208	-0.0294	0.0329
	(0.0123)	(0.0169)	(0.0304)	(0.0387)
Gross Value	0.00255	-0.0106	-0.00612	-0.00368
Added	(0.00766)	(0.00965)	(0.0177)	(0.0275)
Employment Rate	-0.0547*	-0.0533	-0.00862	0.310*
	(0.0327)	(0.0865)	(0.0993)	(0.183)
L.Outcome		0.0846		0.214***
		(0.114)		(0.0599)
Constant	0.104**	0.0713	0.0192	-0.310*
	(0.0349)	(0.0991)	(0.0788)	(0.172)
Observations	966	966	966	966
AME (Fem Majority)	0.0884	0.266	0.0284	0.179
AME (Male Majority)	0.0245	0.151	0.0889	0.278
Autocorr. Coeff.		0.251		0.832
Hansen Test		0.0328		0.0594

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW bite from ASHE, other variables from LFS. Analytical weights. Includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices, region-industry level GVA and year fixed effects. 41 instruments each in columns 2 and 4..

Table 35: Effect of minimum wages on unsocial hours and allowances for them, female-majority vs male-majority industries

	Unsocial Hours		Allowances for Unsocial Hours	
	FE	Arellano-Bond	FE	Arellano-Bond
NMW/NLW bite	0.382**	0.943**	0.0798	0.121
	(0.171)	(0.480)	(0.120)	(0.342)
- squared	-0.0174	-0.135	-0.0101	-0.140
	(0.154)	(0.368)	(0.0889)	(0.172)
Female majority	-0.248**	-0.0312	-0.0383	0.125
	(0.0864)	(0.172)	(0.0712)	(0.146)
Female majority	0.728**	-0.0398	0.125	-0.432
× Bite	(0.267)	(0.527)	(0.207)	(0.424)
Female majority	-0.509**	0.120	-0.0895	0.350
× Bite squared	(0.199)	(0.398)	(0.147)	(0.301)
Graduate share	-0.168***		-0.0546	
	(0.0437)		(0.0410)	
Gross Value Added	0.0949***		0.0271**	
	(0.0236)		(0.0131)	
Employment Rate	0.258*		0.107	
	(0.139)		(0.120)	
L.Outcome		0.0668		0.0606
		(0.0625)		(0.0659)
Constant	0.0357	-0.115	-0.0939	0.0128
	(0.113)	(0.163)	(0.0893)	(0.170)
Observations	966	966	966	966
AME (female majority)	0.508	0.886	0.0902	-0.0715
AME (male majority)	0.362	0.788	0.0682	-0.0392
Autocorr. Coeff.		0.940		0.994
Hansen Test		0.00356		0.101

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW bite from ASHE, other variables from LFS. Analytical weights. Includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices, region-industry level GVA and year fixed effects. 23 instruments each in columns 2 and 4..

Table 36: Effect of minimum wages on Saturday work, female-majority vs male-majority industries

	FE		Arellano-Bond	
NMW/NLW bite	0.629***	(0.181)	0.559	(0.395)
- squared	-0.274*	(0.157)	-0.443*	(0.265)
Female majority	-0.151*	(0.0768)	0.116	(0.151)
Female majority × Bite	0.417*	(0.235)	-0.461	(0.466)
Female majority × Bite squared	-0.268	(0.174)	0.403	(0.351)
Graduate share	-0.247***	(0.0436)		
Gross Value Added	0.0921***	(0.0204)		
Employment Rate	0.101	(0.128)		
L.Saturday work			0.364***	(0.0685)
L2.Saturday work			0.219***	(0.0512)
Constant	-0.0482	(0.102)	-0.0514	(0.161)
Observations	966		879	
AME (female majority)	0.426		0.0526	
AME (male majority)	0.316		0.0515	
Autocorr. Coeff.			0.238	

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW bite from ASHE, other variables from LFS. Analytical weights. Includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices, region-industry level GVA and year fixed effects. 23 instruments in column 2, Hansen test statistics could not be estimated.

Table 37: Effect of minimum wages on flexitime and other worker-led flexible schedules, female-majority vs male-majority industries

	Flexitime		Other Worker-Led	
	FE	Arellano-Bond	FE	Arellano-Bond
NMW/NLW bite	-0.178	-0.129	-0.00733	-0.211
	(0.155)	(0.365)	(0.0567)	(0.178)
- squared	0.0899	-0.0204	0.00630	0.127
	(0.126)	(0.211)	(0.0458)	(0.109)
Female majority	0.236**	0.572***	0.191***	0.160
	(0.0807)	(0.172)	(0.0499)	(0.110)
Female majority	-0.671**	-1.623**	-0.562***	-0.446
× Bite	(0.234)	(0.501)	(0.141)	(0.308)
Female majority	0.468**	1.129**	0.400***	0.300
× Bite squared	(0.166)	(0.359)	(0.0976)	(0.211)
Graduate share	0.0854**	0.0637	0.000249	0.0290
	(0.0401)	(0.0610)	(0.0176)	(0.0384)
Gross Value Added	-0.00273	0.0337	0.0243**	0.0340*
	(0.0179)	(0.0368)	(0.00970)	(0.0175)
Employment Rate	-0.0188	0.0747	0.0223	0.106
	(0.115)	(0.207)	(0.0676)	(0.155)
L.Outcome		0.203**		0.173
		(0.0703)		(0.109)
Constant	0.120	0.0531	0.0388	0.0337
	(0.110)	(0.192)	(0.0474)	(0.115)
Observations	966	966	966	966
AME (Fem majority)	-0.211	-0.483	-0.105	-0.168
AME (Male majority)	-0.0755	-0.152	-0.000111	-0.0658
Autocorr. Coeff.		0.285		0.296
Hansen Test		0.171		0.00276

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW bite from ASHE, other variables from LFS. Analytical weights. Includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices, region-industry level GVA and year fixed effects. 41 instruments each in columns 2 and 4.

Table 38: Robustness checks for zero-hours contracts

	Second Lag		Reduced covariate vector	
L.Zero Hours Contract	-0.0915	(0.185)	0.0500	(0.100)
L2.Zero Hours Contract	-0.0726	(0.143)		
NMW/NLW bite	-0.516**	(0.185)	-0.234	(0.145)
- squared	0.434**	(0.155)	0.339**	(0.119)
Gross Value Added	-0.0108	(0.0117)	-0.0231	(0.0201)
Employment Rate	-0.0845	(0.0692)	-0.147	(0.128)
Graduate Share	-0.000650	(0.0154)	0.0194*	(0.0103)
Post-2013			0.0113***	(0.00267)
Constant	0	(.)	0.145	(0.0935)
Observations	879		967	
Year dummies	Yes		No	
Av. Marg. Effect	-0.0128		0.0920	
Autocorr. Coeff.	0.686		0.567	
Hansen Test	0.102		0.479	
Sargan test	0.124		0.446	
Number of instruments	35		15	

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Includes analytical weights, (1) includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices, region-industry level GVA and year fixed effects.

Table 39: Effects of the National Living Wage introduction on selected outcomes (fixed-effects regression)

	Flexitime	Zero-Hours Contracts	Unsocial Hours
2016	-0.00381	0.0108*	0.0143
	(0.0114)	(0.00557)	(0.0130)
2016 × NLW pre-coverage	0.00321	-0.0226	-0.259
	(0.135)	(0.0529)	(0.178)
Squared	0.0323	0.0358	0.793*
	(0.358)	(0.142)	(0.435)
Graduate Share	0.0484	0.0733*	-0.153
	(0.105)	(0.0401)	(0.139)
Gross Value Added	0.0154	-0.0456	-0.192
	(0.113)	(0.0351)	(0.178)
Employment Rate	0.308	-0.418**	-0.204
	(0.382)	(0.154)	(0.566)
Constant	-0.0503	0.313**	1.026**
	(0.325)	(0.118)	(0.493)
Observations	144	144	144

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW pre-coverage from ONS calculations based on ASHE, other variables from LFS. Analytical weights. Includes the full covariate set (cf. main results in section 3.3.2) and region-industry fixed effects.)

Table 40: Effects of the National Living Wage introduction on selected outcomes, simplified specification

	Flexitime	Zero-Hours Contracts	Unsocial Hours
2016	0.0123	0.00625**	-0.000283
	(0.00858)	(0.00304)	(0.00898)
NLW pre-coverage	-0.276***	-0.0247	0.291**
	(0.0684)	(0.0290)	(0.113)
2016 × NLW pre-coverage	0.0768**	-0.00841	-0.0339
	(0.0378)	(0.0206)	(0.0594)
Graduate Share	-0.0335	-0.0125	-0.287**
	(0.0739)	(0.0222)	(0.0915)
Gross Value Added	0.138	0.0132	-0.154
	(0.115)	(0.0435)	(0.129)
Employment Rate	0.336	-0.159	0.0391
	(0.519)	(0.182)	(0.525)
Constant	-0.210	0.109	0.493
	(0.361)	(0.123)	(0.359)
Observations	144	144	144

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW pre-coverage from ONS calculations based on ASHE, other variables from LFS. Analytical weights. Includes gross-value added, employment rate, graduate, youth and apprentice shares as covariates as well as region and industry fixed effects.

Table 41: Effects on schedule disamenities, using bite with respect to the 30th percentile

	Zero-Hours Contracts		Unsocial Hours	
	FE	Arellano-Bond	FE	Arellano-Bond
NMW/NLW bite	-0.301***	-0.482**	0.442**	-0.449
	(0.0678)	(0.186)	(0.219)	(0.573)
- squared	0.220***	0.342***	-0.0982	0.406
	(0.0472)	(0.103)	(0.167)	(0.464)
Graduate Share	-0.00147	0.00333	-0.187***	-0.181***
	(0.0125)	(0.0166)	(0.0431)	(0.0457)
Gross Value Added	0.00102	-0.00355	0.0924***	0.0462
	(0.00793)	(0.00895)	(0.0225)	(0.0482)
Employment Rate	-0.0298	-0.0109	0.335**	-0.189
	(0.0308)	(0.0608)	(0.140)	(0.206)
L.Outcome		0.0730		0.184**
		(0.109)		(0.0888)
L2.Unsocial hours				0.0796
				(0.0552)
Post-2009				0.0163***
				(0.00409)
Observations	966	966	966	879
Av. Marg. Effect	0.0148	0.00952	0.301	0.134
Autocorr. Coeff.		0.484		0.401
Hansen Test		0.583		0.0916
Number of instr.		35		16

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. (4) includes a single post-2009 dummy, the share of graduates, youth and apprentices, employment rates and region-industry level GVA. (1) - (3) additionally include controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, unemployment rates and year fixed effects.

Table 42: Effects on schedule amenities, using bite with respect to the 30th percentile

	Flexitime		Other Worker-Led	
	FE	Arellano-Bond	FE	Arellano-Bond
NMW/NLW bite	-0.281*	-0.568	-0.124*	-0.421**
	(0.168)	(0.402)	(0.0709)	(0.169)
- squared	0.118	0.211	0.0666	0.176
	(0.117)	(0.264)	(0.0485)	(0.108)
Graduate Share	0.0737*	0.0749	-0.0139	0.0236
	(0.0405)	(0.0559)	(0.0173)	(0.0390)
Gross Value Added	0.00647	0.0478*	0.0330***	0.0320*
	(0.0181)	(0.0287)	(0.00950)	(0.0173)
Employment Rate	-0.0348	0.0554	0.00607	0.244
	(0.114)	(0.211)	(0.0684)	(0.152)
L.Outcome		0.273***		0.375**
		(0.0684)		(0.166)
Observations	966	966	966	966
Av. Marg. Effect	-0.111	-0.265	-0.0278	-0.168
Autocorr. Coeff.		0.157		0.502
Hansen Test		0.173		0.0180

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. (4) includes the share of graduates, youth and apprentices, employment rates and region-industry level GVA. (1) - (3) additionally include controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, unemployment rates and year fixed effects. 35 instruments used in columns 2 and 4..

Table 43: Zero-hours contracts versus casual work as an outcome, fixed-effects and dynamic specifications

	Casual, not on a ZHC		Casual *or* ZHC	
	FE	Arellano-Bond	FE	Arellano-Bond
NMW/NLW bite	0.0361	0.0106	-0.177**	-0.129
	(0.0220)	(0.0778)	(0.0564)	(0.148)
- squared	-0.0431**	-0.0136	0.178***	0.262**
	(0.0183)	(0.0349)	(0.0467)	(0.0925)
Graduate Share	-0.00129	0.00529	0.00820	0.0253
	(0.00807)	(0.0119)	(0.0139)	(0.0180)
Gross Value Added	0.00356	0.00916	0.00115	-0.00346
	(0.00321)	(0.00566)	(0.00781)	(0.00999)
Employment Rate	-0.0382	-0.0312	-0.0905**	-0.107
	(0.0264)	(0.0508)	(0.0416)	(0.0865)
L.Outcome		0.127**		0.0146
		(0.0505)		(0.0868)
Constant	0.0216	0.0148	0.101**	0.0537
	(0.0193)	(0.0477)	(0.0384)	(0.0748)
Observations	966	966	966	966
Av. Marg. Effect	-0.0133	-0.00492	0.0267	0.171
Autocorr. Coeff.		0.282		0.518
Hansen Test		0.769		0.838

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW bite from ASHE, other variables from LFS. Analytical weights. Includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices, region-industry level GVA and year fixed effects. 35 instruments used in columns 2 and 4..

Table 44: Zero-hours contracts versus casual work as an outcome, fixed-effects and dynamic specifications

	ZHC *and* other temporary		ZHC and permanent	
	FE	Arellano-Bond	FE	Arellano-Bond
NMW/NLW bite	0.00196	0.00460	-0.191***	-0.141
	(0.0121)	(0.0308)	(0.0485)	(0.124)
- squared	-0.0000982	-0.0135	0.194***	0.239**
	(0.0103)	(0.0186)	(0.0400)	(0.0734)
Graduate Share	0.00267	-0.000973	-0.00254	0.00667
	(0.00271)	(0.00391)	(0.0103)	(0.0119)
GVA	-0.00136	-0.00154	0.00521	-0.00359
	(0.00138)	(0.00227)	(0.00602)	(0.00729)
Employment Rate	-0.00592	-0.0231	-0.0417	-0.0361
	(0.00880)	(0.0146)	(0.0252)	(0.0537)
L.Outcome		-0.0701		0.0991
		(0.116)		(0.104)
L2.Outcome		-0.136**		
		(0.0683)		
Constant	0.00320	0.0215	0.0708**	0.0238
	(0.00819)	(0.0159)	(0.0247)	(0.0618)
Observations	966	879	966	966
Av. Marg. Effect	0.00185	-0.0108	0.0316	0.132
Autocorr. Coeff.		0.327		0.858
Hansen Test		0.00811		0.986

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW bite from ASHE, other variables from LFS. Analytical weights. Includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices, region-industry level GVA and year fixed effects. 35 instruments used in columns 2 and 4.

Table 45: Effect of minimum wages on schedule amenities and disamenities, with 2-way clustering

	(1) Flexitime, FE	(2) Other Worker-Led, FE	(3) Zero-Hours Contracts, FE	(4) Annualised Hours, FE
NMW/NLW bite	-0.408**	-0.209**	-0.213***	0.0352
	(0.147)	(0.0739)	(0.0560)	(0.121)
- squared	0.266**	0.166**	0.221***	0.0362
	(0.116)	(0.0557)	(0.0464)	(0.104)
Graduate Share	0.0662	-0.0149	0.00927	-0.0385
	(0.0421)	(0.0181)	(0.0125)	(0.0305)
Gross Value Added	0.00761	0.0324**	-0.00240	-0.00193
	(0.0191)	(0.00955)	(0.00767)	(0.0172)
Employment Rate	-0.0240	0.0134	-0.0520	0.00125
	(0.117)	(0.0675)	(0.0329)	(0.100)
Constant	0.188*	0.105**	0.0798**	0.0381
	(0.113)	(0.0460)	(0.0322)	(0.0783)
Observations	966	966	966	966
Av. Marg. Effect	-0.103	-0.0189	0.0396	0.0767

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. NLW bite from ASHE, other variables from LFS. Analytical weights. Includes controls for cell composition with respect to age group and gender, presence and ages of children, educational attainment, non-UK nationality, employment and unemployment rates, share of youth and apprentices, region-industry level GVA and year fixed effects.

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